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THE ACQUISITION OF SKILL IN INFANCY*

From the Clinic of Child Development, Yale University

H. M. HALVERSON

The scarcity of information of a specific nature on the manner in which infants early acquire motor skill[1] prompts the writer in undertaking an investigation which will show how infants attain proficiency in one of the manual skills. Of all gross skills acquired in childhood, proficiency in the use of the hands perhaps develops earliest in life. Prehension attains a state of development at 60 weeks which closely resembles adult prehension. In spite of the great disparity in their structural and functional development, a comparison of the *manner in which infants of 60 weeks and adults manipulate objects* of various shapes reveals no outstanding difference in grasping (14, p. 59).

The importance of the arm and hand in the acquisition of early perceptions of space and mass is amply discussed in an earlier paper (13, pp. 113-119, 254-260). It is undoubtedly because of its advantage of location at the end of a long limb, its generous equipment of cutaneous sense-organs, its capacity for high coordination with vision, its freedom of operation, and its flexibility by virtue of the independent action of its five deft digits that the hand early attains a place of major importance in infancy. Rapid increase in the development of motor skill by the hand is absolutely essential to normal growth as it is only by such development that the infant can acquire adequate knowledge of his environment. Backwardness in development of skill in the use of this prehensile organ must necessarily result in retardation of mental development.

Skilled movements, such as reaching, are on the boundary line between voluntary and automatic action. Reaching starts as a slowly acquired voluntary movement and during development becomes more and more automatic in accordance with the physiological law of facilitation. The infant who, after seeing an object placed before

*Accepted for publication by Carl Murchison of the Editorial Board and received in the Editorial Office, February 2, 1933.

[1]Studies in motor skill in early infancy are cited in an earlier study (13, pp. 281-284).

him, attends another activity while he completes the entire reaching movement accurately has not only reached a high state of proficiency in reaching but his movements are so highly automatized with respect to this type of activity that he requires no visual aid during the progress of the approach in attaining the object. The fact that unattended accurate reaching occurs in the first year of life (13, p. 222) shows that in this type of motor skill eye-hand coordination has developed to the point where the infant may engage in motor activities which require a considerable degree of refinement of visuomotor action, i.e., building block towers, bridges, etc.

A study of the acquisition of motor skill in infants differs fundamentally from a similar study in adults in that infants have no large reserve stock of learned motor acts upon which they may draw in the building-up of a skilled movement. Activities of infants of the age (12 weeks) at which this study begins are still strongly dominated by reflex movements. Development will not occur by means of a remolding into a new form a set of practiced motor acts, but by the modeling of a new movement from acts which are in the main yet to be acquired. The process then is in large part a modification and fusion of a set of reflex and acquired activities, related and unrelated functionally, into a fluent movement cortically controlled.

Inasmuch as our investigation is a study of skill in reaching and this activity attains a high degree of perfection in the first year of life (14, p. 61), we confront the infant with a relatively difficult situation. First of all, the situation must induce the reaching activity. One accomplishes this end by placing before the infant an object of which he desires possession. Secondly, the object must be sufficiently large so that it is easily visible and small enough so that the infant will direct the reaching movement toward the total object and not toward some portion of it. The pellet which measures 7 mm. in its horizontal diameter and 3 mm. in height fulfills these requirements with respect to lure and size. The use of an object of this size lends validity to our data because, inasmuch as the pellet roughly approximates a point in space, one obtains fairly accurate measurements of the peregrinations of the hand in reaching movements with respect to the object.

At 12 weeks the 1-inch cube is substituted for the pellet for the following reasons. At this age no infants reach for the pellet and only three of six infants give overt indications that they see it, whereas all of them regard a larger object (13, pp. 133-135). The study

is an investigation not of regard, as such, but of reaching. Large objects in plain view induce reaching movements in infants who do not react to small objects which are not plainly visible. Under the conditions of our experiment the size of the cube for a 12-weeks infant is not incomparable with the magnitude of the pellet for older infants with respect to visibility. Finally, at this age reaching movements never lead to success in contacting with the object and, regardless of the size and form of objects, no differentiation in grasping occurs which might alter the direction of the reaching activity. (See p. 27.)

Concretely, the investigation deals with the following problems: the accuracy of reaching movements in infancy, the course of accurate movements, the nature of earliest reaching movements and their development, the nature of errors in reaching, whether any uniformity in the nature of the errors at a given age occurs, the direction of the errors, the nature of their corrections, the transformation of reaching patterns, the speed of movement; and the relative usefulness of the parts of the arm and trunk in reaching activities.

Method, Subjects, Procedure

The method of seating the child before the table, the presentation of the pellet, and the study of the cinema film are identical with the procedure detailed in full and followed in the earlier study of prehension (13, pp. 122-132).

Eight infants who come regularly for the normative examinations (9) serve throughout as subjects for our study. Six infants compose the group at 12 weeks. The other two infants join the group at 16 and 20 weeks. After this time the only absentees are CB at 52 weeks, EE at 56 weeks, and CB, HE, and JL at 60 weeks.

We present herewith a brief description of each infant in so far as it pertains to motor activity of the arms. Six infants have long nimble fingers. HE and JF have rather pudgy hands and digits which function somewhat clumsily in grasping. CB is a large and active infant who reaches vigorously for objects at an early age. BB is slender and very active. Her speed in plunging forward for objects is often a serious handicap to accurate reaching. JC is a large infant who moves nicely in reaching. EE often frets but she is deliberate and accurate in the use of her hands. JL is just fair in reaching. At times he does very well, at times he does poorly. BM

executes arm movements nicely His hands and fingers function particularly well in manipulation of objects

The younger infants sit in a small morris chair with a wide canvas belt about the lower chest. When they are 40 weeks old, they sit unassisted before the table. The pellet is placed on the table before the infant at a distance of 6½ inches from the near edge. If the pellet is not procured within 9 seconds, it is moved to a point 3¼ inches from the near edge and the infant is given 10 additional seconds to reach the pellet. A motion-picture camera records the infant's behavior during the situation [2]

The infant begins this experiment with little practice in any form of motor skill, hence the acquisition of skill in his case is an entirely new experience. He receives no training as such in reaching. During his examination, while he is seated at the table, several objects, varying greatly in size, are presented at intervals at the same point, i.e, 6½ inches immediately in front of him. Therefore, his training, which consists largely in the development of motor coordination in its relation to visual space, occurs as a result of his reactions to objects about him in the home.

Method of Study

The cinema records are studied in motion and by stilling the successive frames of the film. The method is adequately stated in a previous investigation (13, pp. 129-131). Records secured by visual observation of the infants supplement the cinema data.

Discussion of Results

The reaching reaction recorded for each infant is the movement in which the infant gets nearest to the pellet. The movement begins when the hand or any portion of it leaves its position of rest for the pellet and ends when the hand comes to a complete stop or is withdrawn in favor of some other activity (13, pp 162-169). In early infancy an approach may consist of a single movement in one general direction or a series of lateral-medial sweeps on or above the table In later infancy an approach is usually a forward movement which terminates at or near the pellet. In early infancy visual regard for

[2]A full description of the photographic apparatus, the position of the camera with respect to the infant, and the lines on the table top which aid in plotting the course of the infant's hand in reaching appears in earlier studies (13, pp 123-129)

the pellet continues to the end of reaching movement. In later infancy regard for the pellet may be momentarily interrupted during the approach or may shift to another object without seriously interfering with the completion of the reaching movement For example, BB at 56 weeks, after a cursory regard for the pellet, looks at the examiner and then starts reaching for it BB's regard goes back to the pellet only at the completion of the approach These facts concerning reaching in later infancy point to a high central correlation between incoming proprioceptive impulses and outgoing effector impulses, which insures cooperation of the appropriate muscles in the reaching reaction Infants of all ages occasionally have one hand resting on the table while the other hand reaches for the pellet In the case of the younger infants, the resting hand serves as a stabilizing agent for the trunk. With infants of 1 year a hand so placed perhaps exercises some steadying influence on the course of the reaching hand

The Analytic Data

Table 1 presents in full the results of a complete analysis of certain aspects of the reaching movements by the individual infants at 13 successive ages, i.e., 12 to 60 weeks. The table deals particularly with lateral and forward movements of the hand during the reaching activity, the distance to the goal at the completion of the approach, the nearest point to the goal at any stage of the approach, the time in cinema frames (13, p. 130) consumed, and the amount of elbow extension in each case. The initials of the infants appear at the left of the table The figures and descriptive data for each infant are opposite his initials in columns under the several items studied. The 13 age groups are clearly set off and the median, range, and other measures of the items for each group appear in the space below the individual measures of the group

If one considers development of motor skill from the point of view of the number of times infants deviate from a straight line in reaching for the pellet, the range of these measures and the median combine to show that the number of lateral digressions decreases gradually, although irregularly, during the first year of life The large amount of lateral groping below the age of 32 weeks is not surprising, because up to this age infants seldom acquire the elusive pellet. With the exception of HE at 36 weeks no unusual number of digressions occur at the later ages. The great number of digressions by CB in early

TABLE 1

Analytic Data on the Reaching Reactions of the Individual Infants and the Age Groups with Special Reference to Measurements of Movements

Name	No. lat. digressions	Amt lat. digression Out	Amt lat. digression In	Lat. dist. from pellet	Absolute dist to pellet	Nearest point to pellet	Reach-time in frames	Elbow movement in degrees		First move-ment	Move-ment ends	Amount forward projection	
							Age—12 weeks						
CB	14	3.0	0.0	4.5	7.0	6.0	128	90-120		out	out	1.5-1.5	
BB	—	—	—	—	—	—	—	—		—	—	— —	
JC	3	1.0	0.0	6.0	8.0	4.5	44	90-120		out	out	3.0-1.0	
HE	—	—	—	—	—	—	—	—		—	—	— —	
EE	13	0.5	5.0	0.0	5.0	5.0	222	60-90		out	in	0.5-1.5	
JF	2	0.5	0.5	0.5	6.5	6.0	49	110-110		in	out	0.5-0.5	
JL	—	—	—	—	—	—	—	—		—	—	— —	
BM	5	0.5	0.5	7.5	10.5	9.5	43	90-90		out	out	0.5-0.5	
Median	5.0	0.5	0.5	4.5	7.0	6.0	49	30	Total	4 out	4 out	Av. advance	0.2
Range	2-14	0.5-3.0	0.0-5.0	0.0-7.5	5.0-10.5	4.5-9.5	43-222	0-30	Total	1 in	1 in	Terminating median	1.0
												No. of advances	1
							Age—16 weeks						
CB	7	4.5	0.0	5.5	8.0	6.5	42	90-160		out	in	2.0-2.5	
BB	4	1.5	0.5	1.0	2.0	2.0	16	160-170		out	in	4.0-5.0	
JC	1	0.0	0.5	1.5	5.5	5.0	23	140-140		in	in	2.5-1.5	
HE	—	—	—	—	—	—	—	—		—	—	— —	
EE	2	3.5	0.0	1.0	5.5	5.5	65	70-110		out	in	0.5-1.5	
JF	2	2.0	0.5	5.5	6.5	5.0	27	150-180		in	out	2.0-2.5	
JL	2	1.0	1.0	6.5	5.5	5.5	35	90-120		out	out	0.5-1.0	
BM	2	1.5	0.5	3.5	6.0	6.0	105	90-90		out	out	1.5-1.5	
Median	2.0	1.5	0.5	3.5	5.5	5.5	35	30	Total	5 out	3 out	Av. advance	0.5
Range	1-7	0.0-4.5	0.0-1.0	1.0-6.5	2.0-8.0	2.0-6.5	16-105	0-70	Total	2 in	4 in	Terminating median	1.5
												No. of advances	5

TABLE 1 (*continued*)

Name	No lat digressions	Amt. lat. digression Out	In	Lat dist. from pellet	Absolute dist to pellet	Nearest point to pellet	Reach-time in frames	Elbow movement in degrees		First movement	Movement ends	Amount forward projection	
							Age—20 weeks						
CB	1	1.5	0.0	3.0	3.0	2.0	20	120-160		out	out	4.5-5.5	
BB	4	0.0	1.5	0.0	1.5	0.5	30	150-160		out	in	6.5-5.0	
JC	5	0.5	2.0	1.0	1.0	0.5[a]	78	140-160		in	in	3.5-6.5	
HE	6	2.5	1.5	5.0	7.0	5.0	61	90-120		out	out	1.0-2.0	
EE	3	3.5	0.0	6.0	7.5	4.0	26	120-150		out	out	3.0-2.0	
JF	1	0.0	3.0	1.0	5.0	3.5	16	120-150		in	in	3.0-1.5	
JL	2	1.0	2.5	1.0	6.5	4.5	41	100-150		out	in	1.0-2.5	
BM	8	0.0	7.0	6.0	7.5	3.5	50	140-180		in	out	4.0-2.5	
Median	3.5	0.7	1.7	2.0	5.7	3.5	35.5	30	Total	5 out	4 out	Av advance	0.1
Range	1-8	0.0-3.5	0.0-7.0	0.0-6.0	1.0-7.5	0.5-5.0	16-78	10-50	Total	3 in	4 in	Terminating median	2.5
												No of advances	4
							Age—24 weeks						
CB	12	2.0	2.5	1.5	1.5	1.5	140	45-160		out	in	3.5-6.0	
BB	6	2.5	1.0	0.5	0.5	0.0	44	40-170		out	in	2.0-6.5	
JC	4	3.5	6.5	0.0	3.0	0.5	46	60-150		out	in	0.5-6.0	
HE	4	2.5	3.5	6.0	7.0	3.5	59	120-170		in	out	2.5-3.0	
EE	2	3.0	0.5	0.5	0.5	0.5	18	150-150		out	in	6.5-6.0	
JF	1	2.5	0.0	2.0	4.0	2.0	44	120-150		out	out	1.5-2.5	
JL	3	5.0	1.5	3.0	3.0	3.0	23	30-160		in	in	0.5-6.0	
BM	3	4.5	0.5	0.0	0.0	0.0	41	120-160		out	in	3.5-6.5	
Median	3.5	2.7	1.2	1.0	2.2	1.0	44	70	Total	6 out	2 out	Av advance	2.7
Range	1-12	2.0-5.0	0.0-6.5	0.0-6.0	0.0-7.0	0.0-3.5	18-140	0-130	Total	2 in	6 in	Terminating median	6.0
												No of advances	7

TABLE 1 (*continued*)

Name	No lat digressions	Amt. lat. digression Out	In	Lat. dist. from pellet	Absolute dist. to pellet	Nearest point to pellet	Reach-time in frames	Elbow movement in degrees		First move-ment	Move-ment ends	Amount forward projection	
							Age—28 weeks						
CB	8	2.5	1.0	0.0	0.0	0.0	73	60-180		out	in	0.5-6.5	
BB	5	3.0	1.5	2.0	2.0	0.5	63	60-180		out	in	1.0-6.0	
JC	3	2.5	0.5	0.5	0.5	0.5	24	45-150		in	in	1.5-6.5	
HE	9	1.0	1.0	1.0	1.0	0.5	58	160-180		in	in	4.0-6.5	
EE	4	3.5	0.5	0.5	0.5	0.0	44	100-160		out	in	1.0-6.5	
JF	2	0.0	0.5	0.5	0.5	0.5	20	90-160		in	in	3.5-6.5	
JL	7	2.0	5.0	0.5	0.5	0.0	52	90-160		in	in	0.0-6.5	
BM	4	4.5	3.5	0.5	0.5	0.0	25	150-180		out	in	0.0-6.5	
Median	4.5	2.5	1.0	0.5	0.5	0.2	48	70	Total	4 out	0 out	Av advance	5.0
Range	2-9	0.0-4.5	0.5-5.0	0.0-2.0	0.0-2.0	0.0-0.5	20-73	20-120	Total	4 in	8 in	Terminating median	6.5
												No. of advances	8
							Age—32 weeks						
CB	2	0.5	0.5	1.0	0.5	0.5	12	90-170		in	in	1.0-6.5	
BB	3	1.0	2.0	0.0	0.5	0.5	40	140-150		out	in	4.0-6.5	
JC	4	1.0	1.0	0.5	0.5	0.5	28	140-180		in	in	0.5-6.0	
HE	4	2.5	0.5	1.0	1.0	0.5	39	150-180		out	in	2.5-6.5	
EE	5	2.0	0.0	0.0	0.0	0.0	37	150-170		in	in	4.0-6.5	
JF	3	1.5	1.0	1.0	0.5	0.5	29	120-170		out	out	1.0-6.5	
JL	3	6.0	0.0	0.0	0.0	0.0	43	90-170		out	in	0.5-6.5	
BM	2	1.5	1.0	2.0	2.0	0.5	35	170-170		in	in	3.5-6.5	
Median	3.0	1.5	0.7	0.7	0.5	0.5	36	35	Total	4 out	1 out	Av advance	4.5
Range	2-5	0.5-6.0	0.0-2.0	0.0-2.0	0.0-2.0	0.0-0.5	12-43	0-80	Total	4 in	7 in	Terminating median	6.5
												No. of advances	8

TABLE 1 (*continued*)

Name	No lat digressions	Amt lat digression Out	In	Lat. dist. from pellet	Absolute dist to pellet	Nearest point to pellet	Reach-time in frames	Elbow movement in degrees		First move-ment	Move-ment ends	Amount forward projection	
							Age—36 weeks						
CB	2	2 5	0 0	0 5	0 5	0 5	24	70-170		out	in	1 0-7 0	
BB	4	1 0	1 0	0 0	0 0	0 0	45	90-170		in	in	0 5-6 5	
JC	3	4 0	0 0	0.0	0 0	0 0	34	30-180		out	in	0 0-6 5	
HE	8	0 5	0 5	0 0	0 0	0 0	65	120-160		str	in	1 5-6 5	
EE	3	0 5	1 0	0 0	0 0	0 0	41	60-160		in	in	0 0-6.5	
JF	4	1 0	0 5	0 5	0 5	0 5	35	70-180		out	in	1 0-6 0	
JL	4	2 0	0 0	0 0	0.0	0 0	43	20-16		out	in	1 0-6 5	
BM	3	0 5	0 5	0 5	0 5	0 0	49	150-170		in	in	4 5-6 5	
Median	3 5	1 0	0 5	0 0	0 0	0 0	42	100	Total	4 out	0 out	Av advance	5 3
Range	1-8	0 5-4 0	0 0-1 0	0 0-0 5	0 0-0 5	0 0-0 5	24-65	20-150	Total	3 in	8 in	Terminating median	6 5
										1 str.		No of advances	8
							Age—40 weeks						
CB	4	1 5	0 0	0 0	0 0	0 0	28	90-180		out	in	3 5-6 5	
BB	2	1 0	0 0	0 0	0 0	0 0	23	90-150		out	in	3 5-6 5	
JC	3	1 0	0 5	0 0	0 0	0 0	23	90-170		out	in	1 0-6 5	
HE	2	1 5	0 0	0 0	0 0	0 0	36	90-170		out	in	1 0-6 5	
EE	2	1 0	0 0	0 0	0 0	0.0	19	120-160		out	in	4 0-6 5	
JF	3	1 0	0 0	1 0	1 0	1 0	19	110-180		out	out	1 5-6 5	
JL	2	1 0	0 0	0 0	0 0	0 0	22	90-150		out	in	2 0-6 5	
BM	2	0 5	0 5	0 5	0 5	0.5	25	90-160		out	in	2 0-6 0	
Median	2 0	1 0	0 0	0 0	0 0	0 0	23	70	Total	8 out	1 out	Av advance	4 1
Range	2-4	0 5-1 5	0 0-0 5	0 0-1 0	0 0-1 0	0 0-1.0	19-36	40-90	Total	0 in	7 in	Terminating median	6 5
												No of advances	8

TABLE 1 (*continued*)

Name	No lat. digressions	Amt. lat. digression Out	Amt. lat. digression In	Lat. dist from pellet	Absolute dist. to pellet	Nearest point to pellet	Reach-time in frames	Elbow movement in degrees		First movement	Movement ends	Amount forward projection	
							Age—44 weeks						
CB	2	0.5	0.0	0.0	0.0	0.0	16	120-160		str	in	2.5-6.5	
BB	2	0.5	0.0	0.0	0.0	0.0	20	140-160		str	in	3.5-6.5	
JC	2	0.5	0.0	0.0	0.0	0.0	18	90-170		out	in	1.5-6.5	
HE	4	1.0	0.0	0.0	0.0	0.0	25	100-160		out	in	3.0-6.5	
EE	0	0.0	0.0	0.0	0.0	0.0	13	90-160		str	in	3.5-6.5	
JF	2	0.5	0.0	0.0	0.0	0.0	38	120-160		out	in	2.0-6.5	
JL	3	1.5	0.5	0.0	0.0	0.0	20	80-160		in	in	1.0-6.5	
BM	4	1.0	0.0	0.0	0.0	0.0	27	70-150		out	in	2.0-6.5	
Median	2.0	0.5	0.0	0.0	0.0	0.0	20	65	Total	4 out	0 out	Av. advance	4.2
Range	0-4	0.0-1.5	0.0-0.5	0.0-0.0	0.0-0.0	0.0-0.0	16-38	20-80		1 in	8 in	Terminating median	6.5
										3 str		No. of advances	8
							Age—48 weeks						
CB	0	0.0	0.0	0.0	0.0	0.0	9	130-170		str	in	1.5-6.5	
BB	2	0.5	0.0	0.5	0.0	0.0	14	120-160		out	in	0.0-7.0	
JC	2	2.0	0.5	1.0	1.0	0.5	12	50-160		out	in	1.0-6.0	
HE	4	2.0	0.0	0.0	0.0	0.0	35	90-180		out	in	2.5-6.5	
EE	2	0.0	0.5	0.5	0.5	0.5	13	120-150		in	in	3.5-6.5	
JF	3	1.0	0.5	0.5	0.5	0.5	20	45-150		out	in	0.0-6.5	
JL	2	0.0	1.0	0.0	0.0	0.0	18	100-150		str	in	1.0-6.5	
BM	3	0.5	0.5	0.0	0.0	0.0	20	90-150		str	in	0.5-6.5	
Median	2.0	0.5	0.2	0.0	0.0	0.0	16	55	Total	4 out	0 out	Av. advance	5.2
Range	0-4	0.0-2.0	0.0-1.0	0.0-1.0	0.0-1.0	0.0-0.5	9-35	30-110		1 in	8 in	Terminating median	6.5
										3 str		No. of advances	8

TABLE 1 (*continued*)

Name	No lat. digressions	Amt. lat digression Out	Amt. lat digression In	Lat dist from pellet	Absolute dist to pellet	Nearest point to pellet	Reach-time in frames	Elbow movement in degrees		First move-ment	Move-ment ends	Amount forward projection	
							Age—52 weeks						
CB	—	—	—	—	—	—	—	—		—	—	— —	
BB	2	0.5	0.5	0.0	0.0	0.0	36	100-130		out	in	4.0-6.5	
JC	2	0.5	0.5	1.0	1.0	0.5	25	90-150		out	in	3.5-6.5	
HE	2	2.0	0.0	0.0	0.0	0.0	27	90-160		out	in	1.5-6.5	
EE	2	0.5	0.0	0.0	0.0	0.0	16	90-140		str	in	2.5-6.5	
JF	2	2.0	0.0	0.0	0.0	0.0	30	90-130		out	in	2.0-6.5	
JL	2	1.0	0.0	0.0	0.0	0.0	24	100-170		out	in	3.5-6.5	
BM	2	0.5	0.0	0.0	0.0	0.0	14	90-150		out	in	2.5-6.5	
Median	2.0	0.7	0.0	0.0	0.0	0.0	25	60	Total	6 out	0 out	Av advance	3.7
Range	2-2	0.5-2.0	0.0-0.5	0.0-1.0	0.0-1.0	0.0-0.5	14-36	30-70		0 in	7 in	Terminating median	6.5
										1 str		No of advances	7
							Age—56 weeks						
CB	2	0.5	0.0	0.0	0.0	0.0	15	110-170		out	in	1.0-6.5	
BB	2	1.5	0.0	0.0	0.0	0.0	13	90-140		out	in	5.5-6.5	
JC	2	1.0	0.0	0.0	0.0	0.0	14	60-150		out	in	0.0-6.5	
HE	0	0.0	0.0	0.0	0.0	0.0	13	90-100		str	in	4.5-6.5	
EE	—	—	—	—	—	—	—	—		—	—	— —	
JF	4	0.5	0.0	0.0	0.0	0.0	40	90-180		out	in	1.5-6.5	
JL	2	1.0	0.0	0.0	0.0	0.0	20	90-150		out	in	2.5-6.5	
BM	3	0.5	0.5	0.0	0.0	0.0	17	45-170		in	in	1.5-6.5	
Median	2.0	0.5	0.0	0.0	0.0	0.0	15	60	Total	5 out	0 out	Av advance	4.1
Range	0-4	0.0-1.5	0.0-0.5	0.0-0.0	0.0-0.0	0.0-0.0	13-40	10-125		1 in	7 in	Terminating median	6.5
										1 str.	0 str	No of advances	7

TABLE 1 (*continued*)

Name	No. lat. digressions	Amt. lat. digression Out	Amt. lat. digression In	Lat dist. from pellet	Absolute dist. to pellet	Nearest point to pellet	Reach-time in frames	Elbow movement in degrees		First movement	Movement ends	Amount forward projection	
						Age—60 weeks							
CB	—	—	—	—	—	—	—	—		—	—	— —	
BB	2	1.5	0.0	0.0	0.0	0.0	22	80-160		str	in	1.0-6.5	
JC	0	0.0	0.0	0.0	0.0	0.0	17	70-170		str	in	1.5-6.5	
HE	—	—	—	—	—	—	—	—		—	—	— —	
EE	2	0.5	0.0	0.0	0.0	0.0	16	70-180		str.	in	0.0-6.5	
JF	2	0.5	0.0	0.0	0.0	0.0	23	80-150		out	in	1.0-6.5	
JL	—	—	—	—	—	—	—	—		—	—	— —	
BM	2	0.5	0.0	0.0	0.0	0.0	27	60-140		out	in	0.5-6.5	
Median	2.0	0.5	0.0	0.0	0.0	0.0	22	80	Total	2 out	0 out	Av. advance	5.7
Range	0-2	0.0-1.5	0.0-0.0	0.0-0.0	0.0-0.0	0.0-0.0	16-27	70-110		0 in	5 in	Terminating median	6.5
										3 str	0 str.	No. of advances	5

infancy arises as the result of vigorous reaching by an unusually active child.

In later infancy digressions do not occur singly. Usually the hand moves out slightly to the side during the approach and then moves in medianly toward the pellet near the conclusion of the reaching reaction. Reaching without some lateral digression does not occur. In four instances the digression is less than 0.5 inch but, inasmuch as no finer measures are used, is recorded as 0.5 inch. This ranking does not distinguish these digressions from the regular 0.5-inch type.

The amount of lateral deviation, according to the median and range of the measures, increases with age up to 24 weeks and then diminishes until 40 weeks. From 40 to 60 weeks the digression is uniformly small. The amount of digression toward the side (out) of the reaching hand also increases up to 24 weeks and then decreases. After 36 weeks this digression is only 1.0 inch or less. Median digressions (in) become greater up to 20 weeks, as infants' activities increase and diminish from this point until 44 weeks, after which time practically no median digressions occur. In early reaching, then, digressions occur with great frequency in both median and lateral directions. Digressions toward the median decrease in amount earlier in infancy than do lateral digressions and remain consistently less in extent throughout infancy. A digression toward the median constitutes a serious obstacle to reaching because the hand obscures *vision of the pellet. Thus, early elimination of this type of error is* an absolute necessity to accurate reaching. Digressions to the side occur normally in all reaching reactions to the pellet.

We find the total average digression for each age (see Figure 1) by adding the amounts of the left and right deviations presented in Table 1 in the two columns under *Amount of lateral digressions*. The steep incline in the curve of digression from 12 to 24 weeks indicates a rapid increase from 2.3 to 5.2 inches in lateral digression of reaching, a fact which, viewed in its own light, is very misleading. In reality the curve does not signify that 24-weeks infants, in comparison with younger infants, have less control over their arm movements. Rather, the sharp incline signals an accelerated increase in arm activity and, inasmuch as this activity is as yet not well under cortical control, the general direction of the movements, because of the prevalence of abductive-adductive shoulder movements at this age, is lateralwise. As a matter of fact, infants of 12 and 16 weeks in most instances actually withdraw the hand from the pellet in

TABLE 1 (*continued*)

Name	No lat digressions	Amt. lat. digression Out	In	Lat dist from pellet	Absolute dist. to pellet	Nearest point to pellet	Reach-time in frames	Elbow movement in degrees		First move-ment	Move-ment ends	Amount forward projection	
							Age—60 weeks						
CB	—	—	—	—	—	—	—	—		—	—	— —	
BB	2	1.5	0.0	0.0	0.0	0.0	22	80-160		str.	in	1.0-6.5	
JC	0	0.0	0.0	0.0	0.0	0.0	17	70-170		str	in	1.5-6.5	
HE	—	—	—	—	—	—	—	—		—	—	— —	
EE	2	0.5	0.0	0.0	0.0	0.0	16	70-180		str	in	0.0-6.5	
JF	2	0.5	0.0	0.0	0.0	0.0	23	80-150		out	in	1.0-6.5	
JL	—	—	—	—	—	—	—	—		—	—	— —	
BM	2	0.5	0.0	0.0	0.0	0.0	27	60-140		out	in	0.5-6.5	
Median	2.0	0.5	0.0	0.0	0.0	0.0	22	80	Total	2 out	0 out	Av. advance	5.7
Range	0-2	0.0-1.5	0.0-0.0	0.0-0.0	0.0-0.0	0.0-0.0	16-27	70-110		0 in	5 in	Terminating median	6.5
										5 str	0 str.	No. of advances	5

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reaching (see p. 24). Hence their lateral errors, viewed with respect to amount of activity and loss of distance to the pellet, are actually greater than the errors of 24-weeks infants (see p 29).

The digression curve from 24 to 60 weeks is perhaps a fair measure of the lateral errors in reaching. The decline in lateral deviations which accompanies forward movements of the hand is rapid up to 44 weeks. Rapid improvement in cortical control over aim movements and possibly in spatial perception accounts for this rapid decrease in lateral digressions from 5.2 to 0.8 inches at this stage of infancy.

The rise in the curve from 0 8 at 44 weeks to 1 1 inches at 48 weeks is relatively insignificant. These infants actually reach with

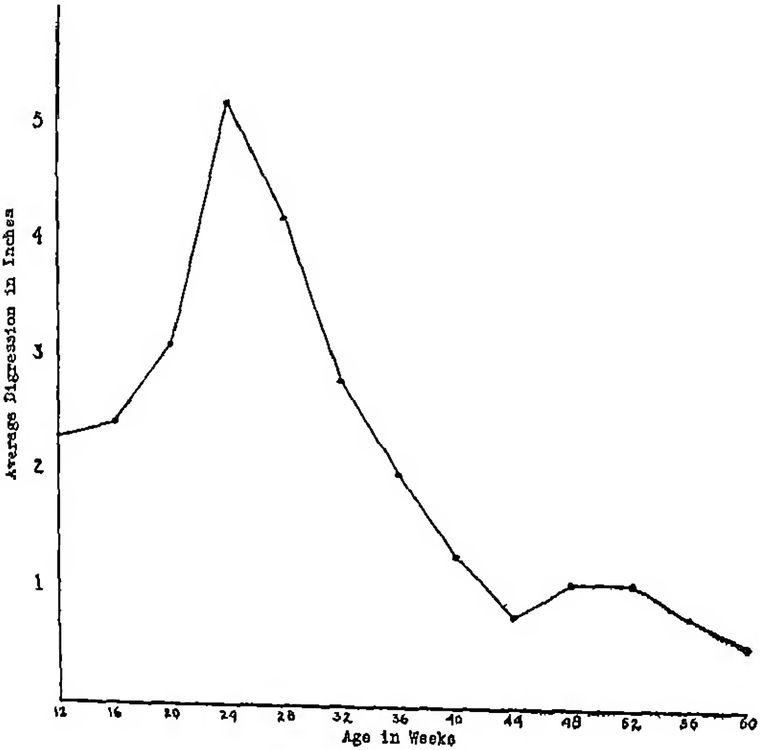

FIGURE 1
TOTAL AVERAGE DIGRESSION LEFT AND RIGHT FROM THE STRAIGHT LINE OF REACHING FOR ALL GROUPS

slightly greater directness at 44 weeks than at 48 and 52 weeks. The fact that at 44 weeks more infants utilize the table top for orientation in reaching (see Table 1) may account in part for the small advantage at this age. The slow decline in the curve for the last two age groups represents a final slight improvement in direct reaching.

In the first half year of life the hand of the infant is often as far from the pellet laterally as medianly at the termination of the approach. The lateral distance is the perpendicular distance from the tip of the forefinger to the median line (of the pellet) At 12 weeks the median measure for the lateral distances is 4 5 inches while the range of the individual measures is 0 0 to 7 5 inches The medians for the succeeding age groups gradually diminish in amount to 0 at 36 weeks, when very few lateral errors in reaching occur. The older infants have little difficulty in the placement of the hand. The range of the individual measures of lateral errors of placement is wide until infants are 28 weeks old, which, by the way, is the age at which the forward projection of the hand equals the distance to the pellet Hence, one finds a positive correlation between ability in forward reaching and ability in the control of lateral movements.

The absolute distance to the pellet from the terminating point of the reaching movement is the length of the straight line between these two points. At 12 weeks the median for this distance is 7.0 and the range of the measures is 5.0 to 10 5 inches Thus none of these infants get very near the pellet The medians at 16 and 20 weeks are also large, but an appreciable drop in the amount of the median occurs at 24 weeks. At 36 weeks and later the median is 0.

Nearest point to pellet signifies the shortest distance from the hand to the pellet at any time during the reaching reaction In the case of the older infants the nearest point to the pellet is always the position of the hand at the termination of the approach With infants from 12 to 28 weeks of age, one finds that in 24 of 36 instances the hand is closer to the pellet at some other time during the approach than at its termination. (Compare absolute distance with nearest point.) At 20 weeks, when infants begin to exercise great vigor in arm movements and reaching is more lateral than forward, the hand often sweeps by the pellet during the approach and stops at a remote point The data show that at this age all infants are farther from the pellet at the end of the reaching movement than they are at some other time before the arm ceases its activity This statement holds for 5 of 8 infants at 28 weeks. At 32 weeks, however,

only two infants fail to better their position with respect to the pellet by continued reaching. From 36 to 60 weeks, with few exceptions, infants are nearer the pellet at the close of the approach than at any other time.

Reaching-time refers to the duration (in motion-picture frames) of the reaching reaction. Inasmuch as the speed of the camera is roughly about 16 exposures per second, the time in 1/16 seconds is easily computed.[3] Under the circumstances a finer timing is of little value in our study. Perhaps it will aid our understanding of the time of reaching if we mention here that no infant at 12 or 16 weeks contacts with the pellet. At 20 weeks one infant (JC) touches it but not with the forefinger. At 24 weeks only BB and BM contact the pellet. At 28 weeks all infants succeed in touching it.

The time which an infant consumes in reaching is by no means an adequate measure of his top speed of arm movement. Several factors affect the time of reaching. Even if we disregard the physiological and anatomical factors of growth, one finds inherent differences which apparently make for hasty reaching by one infant (BB) and slow reaching by another (HE). From 12 to 28 weeks the vigor which infants display in reaching varies greatly from individual to individual. Reaching-times are greater for the more vigorous infants. Failure in reaching apparently discourages some infants and stimulates others to further reaching movements. Interruptions in the approach due to changes in direction retard the movement. Nearness and remoteness of the hand from the pellet at the start affect the speed of the approach. Differences in elevation of the hand in early infancy cause differences in time to reaching. The range of elevation after 36 weeks, however, is uniformly narrow. The discontinuity of the reaching movement for the first six months of life retards the speed of the activity. Raising, projecting, and lowering the hand, which are almost sequential units of the approach in early infancy, are integrated into a smoothly continuous approach in later infancy. Precise placement of the radial digits for grasping by the older infants definitely retards the speed of the

[3]The cameras used in this experiment are carefully timed each year so that one knows their speed of action over a given period. One camera starts at a speed of 16.4 frames per second and after a run of 40 seconds ends at a speed of 16.0 frames. The speed of the second camera diminishes from 16.6 frames to 15.9 frames over a similar period of time. With both cameras the decrease in speed is gradual (13, p. 130).

reaching movement at its close In view of these facts, one must regard each reaching-time as a measure only of the speed with which the individual at a given age advances toward the pellet.

In view of the above statements one understands why there is little consistency in the gain (as shown by the median for the groups) in actual reaching-time with advance in age and why the variability of the individual measures within a given group is considerable. We can, however, assemble the age groups into three classes the non-contacting group, consisting of the 12-, 16-, 20-, and 24-weeks infants, the contacting group, consisting of the 28-, 32-, and 36-weeks infants; and a superior contacting group, consisting of the 40-, 44-, 48-, 52-, 56-, and 60-weeks infants The range of the medians for the reaching-time of our first group under this classification is 35-49 frames, the range for the second group is 36-48; and the range for the oldest group is 15-25 One may translate this statement as follows The unsuccessful reachers (the first group) consume from 2 to 3 seconds in reaching for the unattainable pellet; the second group, who succeed in reaching the pellet but grasp poorly (14, pp. 43-45), also consume from 2 to 3 seconds in the approach, the third group, who reach successfully and grasp well, consume from 1 0 to 1 5 seconds in the approach Hence we may conclude that the amount of time consumed in reaching for the pellet undergoes a great reduction from 36 to 40 weeks and that infants of 40 to 60 weeks of age can accomplish the reaching activity in about one-half the time required by the younger infants

The amount of movement displayed by the elbow is not an index to its usefulness in reaching, but the fact remains that an elbow which has a great range of movement possesses the capacity for greater use to the organism than an elbow with restricted extension and flexion. The data on elbow movement are regarded only as a good approximation of the angle formed by the upper arm and forearm at the start and at the termination of the reaching movement We have not as yet established a more adequate plan for the determination of movements of the elbow than that of measuring its angle from the projected cinema image The medians for the age groups show in general that elbow flexion and extension for the three younger age groups are quite restricted and that from 24 to 60 weeks elbow movements are of considerable extent. Table 1 shows that at 24 weeks the elbow is for the first time of more than minor importance in the reaching activity, although this joint does not as yet contribute

strongly to the success of the activity. One concludes, therefore, that amount of elbow action is not always indicative of its usefulness in the reaching movement.

The period of restricted elbow movements at 12, 16, and 20 weeks immediately antecedes a period of wide range in activity by this joint at 24 and 28 weeks. The increased range of movement signifies more than a gain in mobility of action. The elbow executes movements many of which do not necessarily carry the hand nearer the pellet. In fact they often result in over-reaching or over-flanking the mark, which shows that the elbow does not as yet function efficiently. The great amount of extension of the elbow at 24 and 28 weeks also indicates that its action is not fully supported by the other parts of the body which later in infancy function in reaching movements

From 40 to 60 weeks the range of the measures of elbow extension is in general relatively narrow, a fact which suggests that the infants not only exercise better control over elbow movements and gauge more exactly the amount of movement required in the total arm activity but that a genuine improvement in cooperation by the trunk and the joints of the arm has taken place. Thus each part is contributing more equitably its share to the reaching movement. The data in Table 3 support this statement.

The data on the range of elbow extension (60° to 120°) at 12 weeks show that the forearm clings closely to the trunk. The proximity of forearm to trunk breaks down rapidly, however. At 16 weeks evidence of some remoteness of forearm to trunk appears.

After the first six months the amount of extension by the elbow in sitting situations is not a good measure of its mobility or ease of motion. The position of the hand at the start of an approach, especially if the hand is near the pellet, may be such that very little extension by the elbow will carry the hand to its goal. On the contrary, if the hand is located at the near edge of the table at the start, a relatively great amount of extension is required for the reaching movement. During the first half year infants seldom contact with the pellet. The numerous arm maneuvers (Table 1, *No. lateral digressions*) which the more persistent infants of less than 6 months direct toward the pellet testify to a magnitude of elbow movement (see *Elbow movement*) which in the light of the better-controlled activity of the older infants must not be regarded as a measure of the mobility of this joint. When an infant is 28 weeks old, the

hand in reaching usually finds its mark (see p. 22). At 40 weeks, reaching is quite accurately accomplished (see pp 8 ff). The arm is now a manipulatory as well as a grasping mechanism which practices both temporal and spatial economy in its reaching movements (see pp 8 ff.) Mobility of elbow then should be considered not only in the sense of amount of elbow movement, but also from the standpoint of distance from hand to pellet, number of lateral digressions, success of the approach, and time required for the entire activity.

In this connection reaching from the sitting posture differs greatly from reaching in the dorsal position Infants of less than six months spend most of their time in the dorsal position and their early voluntary motor activities are acquired in this position. The change in body posture from lying to sitting brings about a change of 90° in the relative direction of the pull of gravity on the trunk and arm Hence, in view of the low state of infants' motor development in the first six months, regardless of other factors, the pull of gravity alone on the joints of the arm must affect its movements in different ways when the infant lies dorsally and when he sits at a table.

In this experiment the infant's hands are not placed at predetermined positions for the start of the reaching reaction, because we desire least of all any interference with his movements When the hand lies at a remotely lateral position on the table, the initial direction of the reaching movement, in most likelihood, will be toward the median plane, and, conversely, when the hand rests far medianly on the table, the first movements will be away from the median plane In a few instances one finds the hand at these extreme positions at the start of the reaching reactions, but in most cases the hand rests on the table or at the infant's side within easy distance of the trunk. Our data, therefore, are of some value in the determination of preferential direction of initial movements in reaching

A record of the direction of the initial movement reveals that for at least the first 32 weeks of life the direction is as frequently medianward as it is lateralwise After 32 weeks the trend of initial movements is lateral or straightforward At an early age terminating movements show no definite directional trend At 24 weeks, however, most reaching movements end in sweeps toward the median plane, and from 28 to 60 weeks a movement toward the median plane at the end of the advance on the pellet is definitely integrated in the reaching reaction

Under *Amount of forward projection* we find two columns of

figures both of which refer to forward distances with reference to the table top. Zero is at any point at the edge of the table nearest to the infant. The first and second columns indicate respectively the starting-point and the terminating-point of the reaching movement for each infant Below each age group we list in order the average (advance) gain in forward projection of the hand as measured by the forefinger tip, the median for the ultimate distance, and the number of infants who register a positive gain

The data on the average advance made by each of the 13 age groups show that at 12 weeks the infants actually register a loss (—0.2 inch) instead of a gain during the reaching reaction. The hand has increased the distance to the pellet At 16 and 20 weeks the data show slight gains of less than 1 inch. At 24 weeks, however, an outstanding gain in forward reaching appears. At 28 weeks and at later ages the gain in distance carries the hand forward to the pellet At 12 weeks only one of five infants registers a gain forward in reaching At 16 and at 20 weeks five of seven infants and four of eight infants respectively register gains. At 24 weeks only one of eight infants (EE) shows a loss in distance, and her reaching activity began from a point equidistant with the pellet and ended ½ inch short of it on a sweep across the table. From 28 to 60 weeks all infants register gains. The median measures of the forward position of the hand at the termination of the approach increase slowly from 1.0 inch at 12 weeks to 2.5 inches at 20 weeks and then jump quickly to 6 0 inches at 24 weeks, after which all medians are 6.5 inches—the distance to the pellet.

Approaches which fall short of their mark prevail in early reaching movements and occur to a certain extent during the first year of life Usually the arm swings in circuitously across the median plane from a lateral point so that the hand "cuts in" short of the pellet From 12 to 24 weeks these errors of coordination or judgments of distance are large At 28 weeks and later the errors are small and are corrected by readjustments of the arm so that contact with the pellet always occurs The prevalence of this type of error through the first year is indicated in Table 2

Infants seldom reach beyond the pellet (see Table 2) At 28 weeks, when for the first time all infants can advance the hand to the pellet but lack practice in such remote reaching, three of eight infants over-reach and the remaining five infants "cut in" short of the object

TABLE 2

NUMBER OF APPROACHES IN WHICH THE HAND "CUTS IN" SHORT OF THE PELLET AND THE NUMBER OF APPROACHES IN WHICH THE HAND OVER-REACHES ITS MARK

Age	12	16	20	24	28	32	36	40	44	48	52	56	60
Hand cuts in	5	5	6	8	5	4	4	4	1	3	2	0	0
Over-reaching	0	0	0	0	3	1	0	1	0	0	0	0	0

In this experiment we may classify reaching with respect to the amount of lateral hand deviation which occurs during the progress toward the object We find in a former study (13, pp 175-180) that reaching in its early stages consists mainly of broad lateral sweeps of the hand, while at one year the approach upon the object is quite direct In the first place then, Table 3 classifies the movements with respect to an overhead view Lateral movements are those activities of the arm which carry the hand at right angles to the median plane of the infant's body without any forward advance toward the pellet. If the general direction of hand activity is lateral with some progress toward the pellet, the movement is classified as more lateral than forward. Movements which are more forward than lateral and movements which carry directly toward the pellet form our other categories.

Closer examination of the course of the hand in reaching reveals peculiarities of movement which have interesting developmental significance We refer to the spiral motion of the hand in early reaching and to modifications in elevation of the hand A second classification under *Elevation* in Table 3 shows these peculiarities and the ages at which they occur In sliding movements the hand slips along the table top and remains in contact with it Letter *T* in this column refers to slides which are in fact thrusts. Circular or spiral movements of the hand occur in clockwise direction (as the camera views them) They are seldom performed smoothly because early movements are jerky and abrupt If reaching is entirely circular in character it falls under the category of circular movements When reaching consists of both sliding and circular movements, it is so classified Looping the hand forward refers to movements in which the hand hops or "paws" forward (14, p 35). This movement is in direct contrast with the forward thrust of the older infant who

TABLE 3
BRIEF DESCRIPTION OF COURSE OF HAND IN REACHING

			Age in weeks 12	16	20	24	28	32	36	40	44	48	52	56	60
Overhead View	Lateral movements only		4	6	6	2									
	More lateral than forward		1	1	2	4	5	4	1					1	
	More forward than lateral					2	3	3	6	4	1	2			
	Forward movements							1	1	4	7	6	7	6	5
Hand Elevation	Sliding movements		2	2	2	2	1	1	1	1					
	Sliding and circular movements		2	3	4	3				2T					
	Circular movements		B	2	L 1	1	L								
	Looping hand forward					L 1	2 2C 2S	3 3S B	2 S C		2	1		1C	
	Thrusting hand forward								3	4 B	5 S	6 S	5 B S	6	5
Continuity of Movement	Discontinuous		5	5	5	4	4	2	2						
	Slightly discontinuous			2	2	3	2	4	2	2	2				
	Continuous but not smooth				1	1	2	2	4	4	2	4	2	1	
	Continuous and smooth									2	4	4	5	6	5
	Ends in brief slide							2	3	2	6	4	2	5	4
	Number of stops	Range	2-13	0-7	0-5	0-10	0-5	0-3	0-2	0	0-1	0	0	0	0
	Where principal deviations occur	Throughout	5	7	6	3	3	1							
		Midway			1	5	2	6	5	3	2	4	1	1	1
		Start					2		1	5	4	3	4	3	3
		End			1		1	1	2		2	1	2	3	1
	Ratio of gain in reaching to lateral deviation of hand	Av	—0.67	—0.05	0.33	0.64	2.46	2.94	4.35	4.68	8.54	7.54	5.11	7.60	11.93
		M V	1.14	0.45	0.82	0.55	1.03	1.53	2.07	1.08	3.09	3.59	2.51	3.06	2.91

projects his hand ahead on a low broad arc toward the pellet. Finally, we classify reaching in terms of *Continuity* of movement. Movements which are interrupted by sudden stops are jerky and discontinuous Cessation of action is usually due to a change in the direction of the movement. However, inasmuch as the arm movements of younger infants (24 weeks or less) generally begin and end abruptly, interruptions may occur without change of direction. Sudden changes in speed of motion also produce discontinuous movements In cases wherein the hand comes to a stop before the reaching act is completed, the movement is slightly discontinuous When changes in speed occur slowly, the movement is *Continuous but not smooth* When the reaching act is completed without appreciable interruption of its speed, the movement is continuous.

The item, *Ends in brief slide,* refers to an interesting and important development in reaching for tiny objects. When the hand attains a position radially adjacent to the pellet, the infant may conclude the reaching activity with a slight sliding movement toward the pellet. This final act in the reaching process occurs frequently with older infants

Table 3 reveals that lateral hand movements dominate the reaching activities of infants up to 20 weeks of age. From 24 to 32 weeks hand movements are generally more lateral than forward, although a number of cases in which the forward movement exceeds the lateral appear. At 36 weeks a definite step in direct reaching is accomplished in that the forward movement now is dominant. At 44 weeks the course of reaching, as seen from above, is practically straight and from this age on continues to improve in directness and steadiness

The use of the table top as a means of guiding the hand to the pellet is fairly characteristic of early reaching, and sliding the hand along the top surface of the table continues as late as 40 weeks (see the forward thrust indicated by the two *T*'s at 40 weeks opposite item, *Sliding movements*). In these two sliding movements which are in fact actually thrusts, the infant projects the hand toward the pellet at so low an elevation that the two ulnar digits glide on the surface of the table One cannot say that the infant does or does not use the table top for the purpose of hand orientation with respect to the pellet.

Perhaps the most common type of reaching movement in early infancy is the combination of sliding and circular movements. The

infant swings the hand about, part of the time along the table top and at other times elevated above it These movements usually are either entirely lateral or principally lateral in character and occur most frequently at 20 weeks.

Some purely circular movements appear at all ages in early infancy up to 28 weeks At 24 and 28 weeks these movements become spiralwise, because the hand is carried ahead during its orbicular maneuvers. The letter *B* in the *Circular* column signifies that an unsteady bobbing motion accompanies the movement and the letter *L* at 24 and at 28 weeks indicates that the hand loops forward during the circular movement and thereby describes a spiral course.

Looping the hand forward commonly occurs at 28, 32, and 36 weeks. The hand usually describes a laterally circuitous course. Sometimes the movement begins or ends with a slide (*S*); sometimes the looping is spiralwise (*C*), and occasionally a bobbing motion (*B*), usually an indication of unsteadiness of muscular coordination, occurs.

The older infants thrust the hand toward the pellet. At 36 and 40 weeks these thrusts frequently are laterally circuitous. After 40 weeks the course of the movement is fairly straight. Occasionally the hand slides part of the way. Unsteadiness (*B*) appears infrequently.

Discontinuous movements by the hand characteristically occur as the result of sudden starting or interruption of action by the shoulder and elbow.

When a misplacement of the hand or an interruption in a movement occurs, a close inspection of the cinema record of the action of the trunk and the joints of the arm at the moment of commitment of the error reveals the offending members of the reaching mechanism A study of the action of readjustment by these joints in correction of the error serves as a check on the validity of the judgment passed on the offending members. In similar manner we study the factors which produce changes in the speed of movements

Our table shows that at 12 weeks all reaching movements are discontinuous. Discontinuous reaching prevails up to 28 weeks, and reaching movements of a slightly discontinuous order occur even at 44 weeks. From 20 to 40 weeks, however, arm movements in reaching slowly but gradually improve in smoothness of action. Abrupt stops and changes in the course of hand activity give way to continuity of action which in its early development gives the im-

pression of slight hesitation or jerkiness (See Table 3 under *Continuous but not smooth*)

Continuous movements which are not smoothly carried out occur when the arm at some point during its reaching activity, usually near the end of its advance, readjusts for some small change in direction of pointing by the hand. From 40 to 60 weeks the development of fluency in continuity of arm movement in reaching proceeds rapidly No interruption to the normal action of the trunk and the joints of the arm in reaching occurs. Hence, as the speed and direction of the reaching movement remains unaltered, the movement is both continuous and smooth It is the writer's contention that after 60 weeks very little improvement in unobstructed reaching occurs

One notes that in the case of very small objects, such as pellets, infants of all ages, with few exceptions, in the culminating (13, p. 194) stage of the reaching movement utilize the table top as a base for final precise orientation of the digits in anticipation of fine prehension of the pellet (14, pp 44-45). Now grasping, as such, an activity which does not concern us in this study, begins at the point at which the digits initiate the act of closure upon the pellet. In the wider interpretation of the term the anticipatory setting of the digits during the approach to approximate the size of the prehensile object constitutes the first stage of grasping. On the other hand, grasping is only a part of the whole reaching process, which, in its wider sense, includes also all sorts of prehensile manipulation of the object following its acquisition by the hand For the purpose of this study, therefore, reaching refers to movements which constitute an approach upon the pellet but not to actual closure of the digits upon the pellet

It is only when infants have attained the age of 32 weeks that these fine adjustments near the end of the reaching act make their appearance Table 3 indicates that two infants at this age, in aiming for the pellet, set their hands with care At 44 weeks, six of eight infants employ this method in reaching and, except for the 52-weeks group, the older infants generally employ the abbreviated slide.

The number of times that the hand of the infant comes to a stop before the completion of a reaching movement varies with the energy expended and in general diminishes as the infant matures Table 3, *No of stops,* shows that in early infancy the number of complete interruptions to the reaching activity ranges from 2 to 13 at 12 weeks

and from 0 to 5 at 28 weeks, where movements involving more than finger action occur. At this early period in infancy, when the hand advances upon the pellet by broad lateral sweeps over or across the table top, attainment of the goal of reaching (grasping the pellet) is seldom successful, and the infant who continues the approach with great vigor makes more discontinuous movements, hence more stops, in the direction of the pellet than does the less active infant. From 24 to 40 weeks the constant decline in the number of interruptions to the movements indicates a steady development in cortical control over reaching. With the exception of one interruption in an approach at 44 weeks, all reaching movements from 40 to 60 weeks are completed without stops.

In studying the course of the hand in reaching for the pellet by means of the plotted route of the forefinger on our map of the table top, we find that the greatest lateral deviations in the reaching movements from 12 to 20 weeks occur throughout the entire activity and that such deviations which in early psychological literature are referred to as random movements persist for 28 weeks of life (see Table 3, *Where principal deviations occur*). These deviations result largely from the abductive-adductive movements of the shoulder which early dominate reaching activities, although elbow movements also figure in these deflections After 20 weeks, when the physiological forces which govern reaching are better organized, the large deviations which in early migrations of the hand are equally great in both right and left directions give way to deviations away from the median line of the table. This change in the nature of the deviations is the direct result of the transformation from reaching which is largely lateral to circuitous reaching (13, pp. 174-179). This type of reaching in which the principal deviation occurs near the middle of the course persists to considerable extent up to 48 weeks (see Table 3, *Midway*). From 40 to 60 weeks the greatest deviation in the course of the approach occurs most frequently at the start of the reaching reaction (see Table 3, *Start*) This initial lateral movement, usually of little extent, marks a definite improvement in motor skill, because an early spatial adjustment of the hand to the pellet obviates to a considerable degree the necessity for additional adjustment during the reaching process and thereby eliminates one of the obstacles to direct reaching. When the greatest amount of deviation occurs near the end of the reaching movement, this deflection in early infancy indicates a gross error in the final

placement of the hand After 40 weeks, however, such deflection signifies that the spatial errors which appear in reaching are relatively small and occur only as the result of a final precise orientation of the hand to the small pellet (see Table 3, *End*) When one considers that at 40 weeks grasping is entirely a digital activity, that the fingertips, the small volar pads of which constitute the gripping surfaces, must be very accurately placed with reference to the pellet, and that the small size of the pellet adds to the difficulties of its prehension, one recognizes the necessity for some slight digital adjustment in the last stage of the reaching act (Compare *Amount lateral digression*, *Lateral distance*, and *Absolute distance*.) Table 1 shows that for a few infants at each age from 44 to 60 weeks this final adjustment constitutes the only error in reaching In this connection one observes that other infants also make these final adjustments (see Table 3, *Ends in sliding movement*) However, inasmuch as errors greater than these final orientations occur at other points in reaching, many infants who commit these final errors are listed in other cells of the table, i e, under *Midway* or *Start*

The difference between the distances from the forefinger to the pellet at the start and at the end of the reaching movement represents the gain in actual advance upon the pellet. If the infant in reaching diminishes the distance to the pellet he registers a gain, if he increases the distance, he registers a loss The ratio of this gain (or loss) in reaching to the lateral deviation of the hand from the straight line (the line from the forefinger at its starting-point to the pellet) is a measure of the infant's motor skill in reaching. In other words, this ratio is a measure of the directness or accuracy of the reaching movement. Reaching, then, amounts to pointing the forefinger at the object during the advance upon it, and, in this sense, the experiment becomes a test of motor precision

Table 3 shows that the *Ratios of the gain in reaching to the lateral deviation* of the hand improve from a negative value at 12 weeks to a high positive value at 60 weeks At 12 weeks three of five infants and at 16 weeks three of seven infants register losses in reaching At 20 weeks the table shows a small average gain of 0 33 However, the individual records show that five of eight infants lose distance in reaching At 24 weeks only one of eight infants registers a loss in reaching From 28 to 60 weeks no individual losses appear. The ratio —0 67 signifies that the hand actually withdraws from

the pellet in reaching for it and deviates as much as 3 inches from the straight line (forefinger to pellet) for a retraction of 2 inches. Similarly, 8.54 signifies that for an advance of 8.54 inches toward the pellet, the hand deviates only 1 inch in the lateral direction. The latter figure, 8.54, at 44 weeks then stands for fair accuracy in prehensory motor skill, while 11.93 at 60 weeks indicates a high degree of accuracy.

Accuracy of reaching, then, improves steadily from —0.67 at 12 weeks to 8.54 at 44 weeks, drops to 5.11 at 52 weeks and rises to 11.93 at 60 weeks. Figure 2 presents graphically the gains in accuracy of reaching. The mean variations of the individual measures in general are fairly large.

The first 60 weeks of life, then, is a period in which motor skill in reaching improves from purely negative results in accomplishment

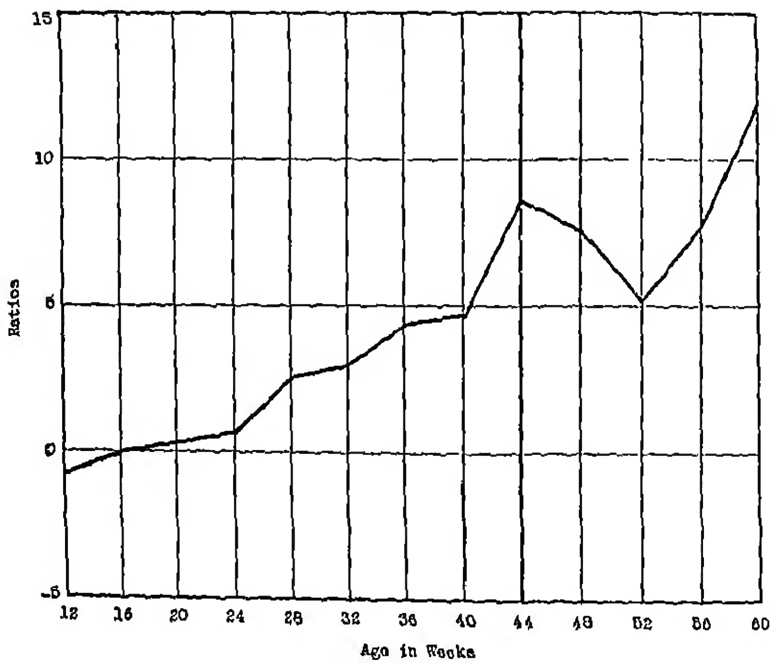

FIGURE 2
RATIO OF GAIN IN REACHING TO TOTAL LATERAL DEVIATION OF HAND—A MEASURE OF THE DIRECTNESS OF THE REACHING MOVEMENT

with gross errors in direction to absolute success in accomplishment with only slight errors of direction In fact, skill in reaching at 60 weeks is practically a matured activity which compares very favorably indeed with adult prehensory skill. A comparison of the reaching activities of 60-weeks infants, 5-year children, and adults[4] shows that all subjects direct the hand with great accuracy, plane the hand toward the pellet at a low altitude, flex the wrist ulnarward, coördinate the individual reaching functions of the trunk, shoulder, elbow, wrist, hand, and digits into an easy fluent movement in which each of the parts contributes adequately to the success of the reaction.

Lack of knowledge in the use of particular objects, such as a pencil or toothbrush, handicaps infants in reaching, because the part of the object toward which one points the hand depends greatly on the use one makes of the object Anatomical differences such as the size of trunk, arm, and digits give the adult an advantage in reaching The posture of the ulnar digits is perhaps of some slight advantage to adults A greater flexion of these digits by adults removes the hazard of interference by objects in the path of the approach

A rough approximation of the relative amount of participation in reaching by the trunk, shoulder, elbow, wrist, and fingers is obtained by ranking the mobile parts in accordance with the value of their contribution to the success of the movement.

We fully realize the impracticability of judgments on the relative value of the movements by parts of a limb in a complicated act as pointing, when such judgments are made on the basis of unaided visual observation The use of cinema records which permit unlimited repetitional performance of a given motor activity at normal and slow speed and by successive "stilling" of the individual frames of the photographic record, however, greatly minimizes the difficulty of such a study.

The determination of the relative usefulness of the component movements in the success of the total motor activity in young infants is a fairly easy accomplishment by reason of the simple fragmentary character of the non-integrated activity and the great differences in the amount of function by the several parts of the body under consideration However, as prehension develops, and the component parts of the total activity are gradually integrated into a smoothly continuous movement, difficulties in the determination of the useful-

[4]The investigation is as yet not ready for publication

TABLE 4
RELATIVE USEFULNESS OF PARTS OF BODY IN REACHING

Age	Trunk	Shoulder	Elbow	Fingers	Wrist
12	3,3,3,3,3 24	1,1,1,1,1 8	2,3,3,1,3, 19	3,3,3,3,3 24	3,3,3,3,3 24
16	3,3,3,3,3,3,3 24	1,1,1,1,1,1,1 8	2,2,3,3,2,1,2 17	3,2,2,3,3,3,2 20	3,3,3,3,3,3,3 24
20	3,3,3,3,3,3,3,3 24	1,1,1,1,1,1,1,1 8	2,3,2,2,2,2,2,3 18	2,2,2,3,2,3,2,2 18	3,3,3,3,3,3,3,3 24
24	3,3,3,3,2,3,3,3 23	1,1,1,1,1,1,1,1 8	2,2,2,3,2,3,2,3 19	2,2,2,3,2,2,3,2 18	3,3,3,3,3,3,3,3 24
28	3,3,3,3,3,3,3,2 23	1,1,1,1,1,1,1,1 8	1,2,2,2,2,2,2,3 16	1,2,2,2,1,2,2,2 14	2,3,3,3,3,3,3,3 23
32	3,1,1,3,2,3,3,2,2 20	1,1,1,1,1,1,1,1,1 8	2,2,2,2,2,2,2,2 16	1,1,2,2,1,2,2,2,2 15	2,2,3,3,2,3,3,2 20
36	2,2,1,2,1,2,3,1 14	1,1,1,1,1,1,1,1 8	2,2,2,1,1,2,1,1 12	1,1,1,2,1,2,3,2 13	1,2,2,3,1 3,2,2 16
40	2,1,1,1,1,2,1,2 11	1,1,1,1,1,1,1,1 8	1,1,2,1,1,2,1,2 11	1,1,1,2,1,2,1,1 10	1,1,1,2,1,2,1,1 10
44	2,2,2,1,1,2,1,1 12	1,1,2,1,1,1,1,1 9	1,1,1 2,1,1,1,2 10	1,1,1,2,1,2,1,1 10	1,1,1,2,1,2,1,1 10
48	1,1,1,2,1,1,1,1 9	1,1,1,1,1,1,1,1 8	1,1,1,2,1,2,1,2 11	1,2,1,2,1,1,1,1 10	1,2,1 2,1,1,1,1 10
52	1,1,1,1,1,1,1 8	1,1,1,1,1,1,1 8	1,1,1,1,1,1,1 8	1,1,2,1,2,2,1 11	1,1,2,1 2,1,1 10
56	1,1,1,1,1,1,1 8	1,1,1,1,1,1,1 8	1,1,1,2,1,1,1 9	1,2,1,2,2,1,1 11	1,1,1,2,2,1 1 10
60	1,1,1,1,1 8	1,1,1,1,1 8	1,1,1,1,1 8	1,1,1,2,1 10	1,1,1,1,1 8

ness of the parts of the body multiply, but are scarcely insurmountable, so long as we do not attempt to classify them too strictly.

Only three classifications therefore appear. We give a rank of *1* to those parts which contribute eminently to the success of the reaching reaction, a rank of *2* to parts which aid materially but not prominently in the movement, and a rank of *3* to parts which cooperate weakly or apparently not at all in the total activity

Table 4 shows at each age the relative extent to which the anatomical parts under investigation cooperate in reaching. The top numbers in each cell of the table are the individual rankings for each infant, the single number in the lower part of each cell is the sum

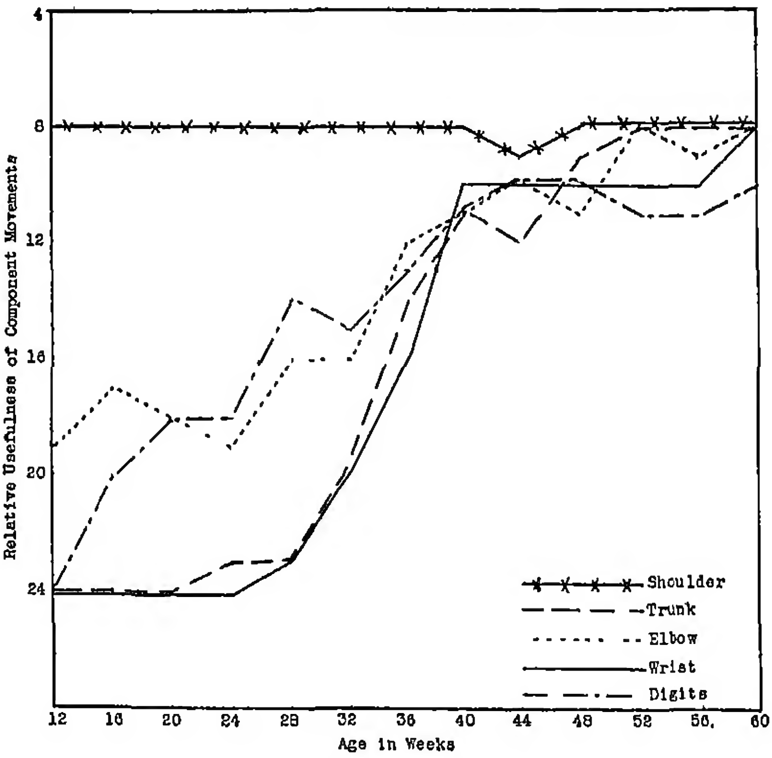

FIGURE 3

RELATIVE VALUE OF THE PHYSIOLOGICAL COMPONENTS OF THE REACHING MOVEMENT

of the individual rankings within that cell. The sum is computed on the basis of a full quota of eight infants at each age in cases wherein the number of infants is less than eight. A graphical presentation of these sums for the several physiological components of the reaching movements appears in Figure 3.

The trunk may aid in reaching activities by leaning forward and by twisting so that one shoulder is rotated ahead of the other. These movements, of course, shorten the distance to the pellet. Inasmuch as body equilibrium is not well established before 36 weeks, infants of this age or less are seated in the chair during the experiment. The belt does not altogether prevent infants from leaning forward and rotating the trunk. Up to 24 weeks infants either sit upright against the back of the chair or lean forward against the belt. At 28 weeks greater trunk movement occurs, with precarious equilibrium. Table 4 shows that active cooperation in reaching by the trunk begins at 32 weeks and develops rapidly for the next 8 weeks. At one year the trunk contributes its full share in this motor activity (see Figure 3)

In infancy, shoulder activity dominates the first voluntary reaching movements. At first its activities are confined largely to abduction and adduction. Gradually flexion (forward), which aids materially in shortening the course of reaching, supplants these movements until at one year their influence in reaching is practically negligible Table 4 shows that the shoulder for at least the first 40 weeks of life contributes more than its share to reaching movements and remains a potent factor in later reaching reactions.

The elbow early renders material aid in reaching. The infant is almost one year old, however, before this joint contributes as much to the success of reaching as does the shoulder, although several instances of rather superior cooperation by the elbow appear from 36 to 48 weeks. At first elbow extension under the condition of our experiment is quite confined. Shoulder movements often carry the arm about with little elbow flexion or extension. Later, when full extension of the elbow occurs, for many weeks the timing of its movements with shoulder and trunk activities is inaccurate enough to cause large errors in reaching. Readjustments in elbow extension usually correct these errors.

Although the fingers begin to function materially in reaching at 20 weeks by opening, closing, and scratching movements, extension which anticipates radial grasping starts at 28 weeks and improves

fairly rapidly to 40 weeks.[5] Pointing by the forefinger becomes firmly incorporated in reaching activities during this period Final adjustments by the digits at the end of the reaching movement, which involve careful placement of the forefinger and thumb, constitute a difficult maneuver, and the period from 40 to 60 weeks *shows slow progress in this fine prehensory activity*

The principal rôle of the wrist in reaching is ulnar flexion which brings the distal volar pads of the forefinger and thumb forward toward the pellet and favors contra-posture of these digits with respect to the pellet In addition, ulnar flexion swings the digits out of the direct line of vision so that the pellet may be seen during the course of the approach *Secondly, the wrist aids by extension* and flexion, whereby the hand is elevated or lowered as the exigencies of the reaching activity may require. The wrist renders little material aid until about 32 weeks. Up to this time the pawing type of reaching requires little cooperative action by the wrist It is not until digital prehension (14, p 56) appears that the wrist takes an active part in reaching Cooperation in the reaching activity by the wrist follows closely on the heels of digital aid. In fact, functional development of the digits in part forces functional development of the wrist. The rise to prominence by the forefinger and thumb in all prehensory activities requires that these digits take the lead in reaching, and this act is best accomplished by ulnar flexion of the wrist One notes (Table 4) that wrist cooperation follows digital participation up to 36 weeks and then closely parallels the latter for the remaining ages

The digits and elbow are of greater aid in reaching up to 32 weeks than are the trunk and wrist. From 28 to 40 weeks the increase in usefulness of the trunk and wrist is rapid From 40 to 60 weeks the trunk, elbow, wrist, and digits render about equal assistance. The shoulder is an important factor in reaching at all ages (see Figure 3)

Review of Related Studies

The literature on child psychology reveals many investigations in the motor skills of children of preschool and school age Measure-

[5]One must not overlook the fact that, as one of the physiological components improves in its reaching functions, the other components also increase in usefulness, hence, while its absolute progress in functional development may be fast, its relative value to the total reaching activity may be slower than that of other components

ments of motor skill as such in infants of one year, however, are scarce, because infants manifest few skills in this period of life and investigators experience difficulties in experimentation

Anderson (1, p. 148) distinguishes early and later motor development in the child In early infancy when the individual possesses a number of relatively simple sensory motor responses, his progress occurs "by the development of a series of acts common in large part to all." Later, when he establishes motor coordination for the performance of the more common activities, "future advance takes place by the development of a wide variety of specific motor habits" (37, pp 289-291).

Numerous scales for the study of motor development in preschool children, studies in special motor skills such as tracing, tapping, etc., and studies on the effects of practice on motor learning (1, pp. 144-162, 17, pp 11-17) appear in the scientific literature on early childhood. In an investigation carried on 40 years ago Hancock (15) furnishes examples of motor skill in young children ranging in age from 5 to 7 years.

Hicks (17) gives an excellent review of the field of development of motor skills in children with special emphasis on acquisition of motor control in target throwing.

The study of the acquisition of motor skill in very young infants presents problems which contrast sharply with the study of this development in older children and adults. In the former case one experiments on subjects whose motor activities develop from spontaneous unlearned movements to voluntarily controlled movements, whereas, in the case of older subjects one deals largely with movements under cortical control. In early manual activities, then, one is confronted with such problems as the nature of the transition of spontaneous and involuntary movements into voluntarily controlled movements, the manner in which cortical control over prehensile activities manifests itself, and the measurement of such control in terms of demonstrable phenomena

The infant has no reserve supply of learned activities upon which he may draw for the establishment of the skilled movement. The acquisition of skill is not a case of refinement of motor action by a given set of structurally mature nerves and muscles It is rather a case of the development of anatomical parts which are structurally and functionally immature and which at first are quite lacking in

ability to combine the component reaching activities into harmonious and reciprocal relationship

At 12 weeks, the age at which this study starts, the infant cannot sit erect, cannot hold his head erect, is only beginning to reach voluntarily for objects, and in some cases probably cannot clearly perceive visually at 6½ inches an object the size of a pellet. The shoulder of this infant, although it is the most tractable of the joints, moves almost entirely in abductive-adductive manner The elbow in reaching never attains full extension and is usually held quite flexed The wrist flexes little in any direction The fingers are generally flexed into the palm and the thumb is the least active member of the digits The tendons, joints, and muscles of the arm, along with their sensory equipment, are at that stage of development wherein they contribute little aid by way of proprioceptive experience in the acquisition of motor skill (16, 31, 34, p 86, 35, pp 5-25, 72-75, 81-93).

A brief review of our facts shows that when the young infant is awake, he is almost constantly in motion. At least some portion of his body is undergoing some activity. The limbs move about in jerky spontaneous excursions and the fingers especially engage in almost ceaseless microkinetic movements These activities of course do not begin at birth They have their inception in early prenatal life

In early postnatal life, if we consider only the activities of the upper limbs, we find that the earliest movements of the shoulder joint consist in jerky abductive-adductive displacements of the arm

The elbow is usually flexed or limply extended The wrist scarcely moves and the forearm-hand relation is relatively fixed so that a line joining elbow, mid-wrist, and medius digit is straight. The movements of supination and pronation of the hand (rotation up and down of the palm), effected by action of the muscles of the forearm which cause the radius to revolve about the ulna, occur only when the infant, dorsally placed, abducts and adducts the arm at the shoulder. When the arm abducts beyond shoulder height, supination occurs, when the arm returns on its adductive phase, pronation takes place. The fingers are usually flexed into the palm with the thumb clasped beneath them or adducted against the radial aspect of the index finger

If the examiner strikes sharply the table on which an infant lies dorsally, the latter's arms abduct violently in a great arc to positions near the head. If no further stimulation is given, the arms will slowly adduct toward their starting-positions.

Returning to the literature of the field, Meyer notes that young infants stretch their arms so that they are either perpendicular to the trunk or extend toward the feet. Under certain more or less violent stimulation they may throw the arms upward The actual course of the movement is not described (22, pp. 135-136).

Dearborn (6) states that indications of voluntary control in reaching appear in the tenth week and even intimates the presence of such control to a slight extent in the third week.

Warner finds that the young infant offers resistance to attempts to extend the elbow. At 1 month the limbs move with greater force and with a greater amplitude and mechanically somewhat more effectively (31, p. 35). At 3 months the senses exercise some control over the arm movements but the hand movements toward desired objects are *not straight*. At 4 months microkinesis can be temporarily inhibited (31, p. 36).

According to Preyer the infant is at least 3 months old before he makes deliberate movements (24, pp. 331-332). For example, first attempts at seizing occur near the end of the fourth month and pointing appears in the eighth month

Warner (31, pp. 33-36) furnishes a good example of the manner in which spontaneous movements persist throughout early life In the waking infant microkinetic movements take place in the limbs, particularly in the fingers These movements occur irregularly but with considerable constancy. At 3 years microkinesis is pretty well controlled by the senses but persists even to 10 years. At 3 years there is also considerable inhibition of many forms of movements

An object which is plainly visible usually sets off the reaching mechanism. Gesell (8, pp. 105-107) states that visual regard for an object results in increased arm activity at 3 months and that incipient arm movements of reaching occur at 4 months.

In general, studies of the motor development of young infants have dealt principally with the enumeration of the various motor responses,[6] the recording of the dates of the appearance of these responses, and the observation of developmental changes of growth (6, 8, 24, 27, 28, 29). Two examples of the last type of study follow. Sherman and Sherman (26, pp. 80-83) study coordination of arm and hand movements in young infants by noting the number of defense arm movements that an infant executes before both of the

[6]Studies of this type are not referred to in this investigation.

infant's hands contact with the tip of the examiner's finger when this digit is pressed against the infant's chin The results indicate improvement in arm coordination with advance in age. At 13 days infants cannot execute immediately accurate defense movements, although older children make correct movements easily The investigators have no measures concerning the course of the movements. Neither do they record the direction of the final pushing force nor mention the part of the infant's hand which contacts with the examiner's finger.

A review of Gesell's study (8, pp 113-134, 236-242) reveals the following facts concerning skill in reaching At 1 month crawling movements appear. At 2 months the dorsal infant makes vertical arm thrusts which do not follow a straight course. At 4 months he scratches and bangs the table At 6 months he reaches more bilaterally than unilaterally At 7 months he tends to reach unilaterally, rotates the hand freely and rakes or scoops in the pellet. At 9 months reaching is unilateral The infant pats the table and claps the hands together by lateral movements. Finally, at 24 months he imitates vertical and horizonal strokes with a crayon

The subjects in our study have at first little appreciation of tridimensional space. It is true that vision in infants is pretty well developed at 12 weeks Saccadic eye-movements, ocular pursuit, eye-coordination, and coordinate compensatory eye-movements function well after the first 6 weeks of life (20, pp. 327-336, 416-418) while coordination of eye and head movements and directing the regard from one object to another are also established before the twelfth week (20, pp. 332-333) However, infants of 12 weeks have little or no experience in perception of visual distance and lack ability in the coordination of eye and arm in relation to external objects Several studies (8, pp. 236-240, 13, p. 174, 24, pp. 325-333; 32, pp 304-310) support these statements Preyer (24, p. 251), for example, reports that young infants reach short for near objects and grasp at objects far out of arm's distance These movements gradually come to an end, but Preyer is unable to set the date of disappearance. Halverson (13, pp. 159, 251-252) states that under-reaching by young infants is common and in the present study (p 23) records the amount of this type of reaching by infants of different ages.

In the early stages in the acquisition of skill, the young infant, as a rule, executes movements which occur in irregular series with re-

spect to both time and space and show evidence of lack of cortical control. When in time he fortuitously hits upon movements which are more effective than other movements toward the accomplishment of the successful act, he utilizes these more effective movements as a basis for further extension of the skill

One might say that the ability of an individual to reach straight for easily prehended objects is a direct measure of the dominance of cortical control over reaching movements Straight projection and placement of the distal pad of the forefinger upon an object at a distance of 6½ inches constitutes a real achievement by the infant Early efforts in reaching result in lateral or up-down movements or combinations of these movements, which are in general at right angles to a straight line from the hand to the object. At one year the course of the reaching hand is straight for the object. All movements which tend to carry the hand at right angles with the direct line of the hand and object (with the exception of elevation for clearance of the table) have been eliminated. The high state of perfection which skill in reaching attains during the first year is scarcely surpassed by any other type of learning during the same period of life.

Awareness of movement of parts of the body comes about through impressions from the proprioceptive sense-organs located in the tissues of the moving parts. In a motor act, such as reaching, visual perception of the successive relative positions of hand and object and proprioceptive experiences of trunk and arm movements of adjustment and readjustment to the position of the object guide and control the progress of the motor act. In the acquisition of skill in the performance of the act modifications of the movements are accompanied by corresponding modifications of the impressions from the proprioceptive sense-organs, and the experience so gained and retained leads ultimately to perfection of accomplishment of the act

In fact, by the time the infant is 48 weeks of age, so well established is the proprioceptive pattern which guides the reaching arm that, having at first visually located the object, he can actually initiate and carry out accurately the entire reaching act, without further regard for the object.

This feat of skill then involves (*a*) gradual improvement in the coordination of muscular movements of the trunk and limbs which function in forward projection of the hand; (*b*) inhibition of movements which tend to carry the hand at right angles to the line of

direct reaching; (*c*) mastery over body equilibrium; and (*d*) fine sensory motor coordination between the senses and muscles which cooperate in the acquisition of the motor skill.

We may not regard the infant as merely an adult in miniature (23, pp 262-263) The increase in length of body, trunk, and arms (2, 5, 25, pp. 417-479, 30, 37) during the first year of life is considerable, so that infants of one year enjoy a distinct advantage over infants of 12 weeks in reaching which demands full arm extension Thus changes in growth of body structure, a relatively insignificant feature in the course of experimentation on older children and adults, is a most important factor in the acquisition of motor skill in young infants.

Not only are the body proportions of the infant and adult greatly dissimilar (23, pp 262-270) but, according to several investigations, the bones and muscles, as well as the nerves, of young infants are numerically and structurally immature [7]

Studies in the anatomy of the nervous system of the human infant reveal that neither the neurons of the cerebral cortex nor the neurons of the peripheral system which function in the voluntary control of arm movements are structurally mature at birth (13, pp 55-59) At 3 or 4 months development within the nervous system has reached a stage in which some cortical control over prehensile activities may be expected.

The transformation of discontinuous lateral arm movements into continuous direct movements in reaching during the first year of life affords an excellent example of the manner in which the cortex gradually assumes control over motor activities The infant of 20 weeks reaches jerkily for objects and in so doing utilizes principally the shoulder The 32-weeks infant employs practically all parts of the arm in reaching activities but his movements are still slightly discontinuous. At 52 weeks the infant projects his arm in a smooth direct course for the object

Then again, within this same time span, the hand of the infant develops functionally from a paw-like appendage into a highly coordinated organ.[8] For young infants whose reaching movements are poorly directed, pawing and corralling, in which the entire hand

[7]Full discussion of this statement appears in our earlier paper (14, pp 48-51)

[8]Note the description of early reaching movements presented on page 23

is called into function, yield greater success in the acquisition of the pellet than does the use of two or three digits (14, pp. 43-46).

At 60 weeks, the age at which this study ends, the infant easily maintains good trunk and head equilibrium. He glides smoothly forward, backward, and laterally without fear or loss of balance and pivots freely in reaching movements. He advances one shoulder and retracts the other in the facilitation of distant reaching, and combines into an accurate and smoothly coordinated arm movement the individual functions of the entire mechanism of the body. In passing, one recalls that the infant is too young for verbal instruction or guidance in this investigation.

If development of motor control occurs first in the larger and more fundamental muscles and then progresses toward the smaller muscles which function in the more refined movements (3, p 120; 4; 12, pp. 9 ff.; 15), we should expect better-controlled activity by the large shoulder muscles than by muscles of the forearm and hand in arm movements of early infancy. Elbow, forearm, and, finally, digital activities should come later under cortical control That increase in skill begins with the larger muscles and extends to the smaller finds support in the observations of students of infant behavior (7; 13, 24; 27; 28; 29; 32, p. 312, 33, p. 86) and in the fact that structural development of nerves and muscles proceeds peripheralward (16; 21, 34, pp 8-25, 72-75, 81-93, 35, pp. 833-834; 36). Similarly, our study shows that the great change in the manner of reaching during the first year of life is the transformation from crude, jerky, lateral movements to direct reaching movements in which the actions of a highly coordinated upper arm are abetted by a very prehensile hand with its many possibilities for varied action and its sensitive finger-tips.

Development of motor skill in reaching movements, then, in addition to being contingent largely upon anatomical and physiological maturation, depends on constant modifications in central correlation of exteroceptive impulses from the retina of the eye and proprioceptive impulses from the moving parts of the trunk and arms as they undergo changes in posture. Unnecessary components of the reaching act in early development which carry the hand away from the direct line of approach to the object are gradually eliminated and components which are essential to success in reaching from the standpoint of spatial economy are retained. The coordination of these essential components into a smoothly continuous act constitutes per-

fection in the reaching movement. Skill in reaching requires that each of the components of the movement contributes its share to the success of the total activity. In perfect reaching the exact amount of the proper type of movement by each part is so timed that all components of the activity integrate automatically into a smoothly continuous and direct motor act

CONCLUSIONS

1. Reaching at 12 to 20 weeks consists of discontinuous lateral arm movements in which the hand slides on the table top, revolves clockwise above the table, or combines both of these activities. Later, the movements become more continuous, the lateral deviations decrease as forward projection increases, and the sliding and circular motions diminish. At 60 weeks the infant's capacity in the perception of visual space and his ability in the coordination of trunk and arm movements are so highly correlated that reaching is accomplished by a smooth, continuous movement of the hand with little or no spatial error.

2. In contrast to acquisition of skill by older children and adults, the infant at the start of this investigation has no repertoire of acquired movements upon which he may draw in the development of the skill He must construct it upon unlearned motor activities With this handicap, however, his skill at 60 weeks compares favorably with adult skill.

3. In the acquisition of skill in reaching during the first 60 weeks of life the first movements are reflexes or simple sensorimotor responses of which the abductive-adductive movements of the shoulder are most prominent The next stage is the period of slowly acquired voluntary movements which range from crude groping activities to direct reaching In the final stage, the movements are largely automatic. As a result of practice the skill is now a more or less definitely fixed motor pattern of response

4 High skill in reaching requires accuracy in perception of visual space and complete synergic control over the muscles which function in the maintenance of sitting equilibrium, in the inclination and twisting of the trunk, and in flexion and extension of the several joints of the arm and hand

5 Lateral deviations of the hand in reaching increase in amount up to 24 weeks and then diminish until 40 weeks, after which time the lateral error is uniformly small. The amount of digression

inward, toward the median plane of the body, becomes greater up to 20 weeks and then decreases, while the amount of digression in the opposite direction increases up to 24 weeks and then gradually decreases After 44 weeks no median digressions occur Lateral digressions, however, occur normally at all ages. The increase in the amount of median and lateral errors from 12 to 24 weeks signifies an advance in arm mobility and not an actual loss in cortical control over arm movements

6. Progress in advancing the hand toward the pellet takes place slowly from 12 to 20 weeks From 20 to 24 weeks a remarkable gain in progress occurs in which the hand almost attains the distance of the pellet At 28 weeks all infants can reach forward to the pellet.

7. The ratio of the gain (or loss) in progress toward the pellet to the lateral deviation of the hand from the straight line of the advance is an excellent measure of the infant's skill in reaching. The data on these measures show that skill in reaching improves gradually with age from —0 67 at 12 weeks to 8 54 at 44 weeks, then declines to 5 11 at 52 weeks, and finally reaches a high mark of 11 93 at 60 weeks.

8. A comparison of the physiological components of the reaching movements shows that for at least the first 40 weeks of life the shoulder functions more effectively in reaching than does the trunk, elbow, digits, or wrist From 16 weeks on, the elbow and digits participate in reaching movements with increasing effectiveness until at 40 weeks they closely approximate the shoulder in usefulness in reaching. The trunk and wrist are functionally retarded. They display little effectiveness until after 32 weeks when they improve rapidly in usefulness in reaching At 40 weeks the trunk and wrist participate as effectively in the reaching movement as do the other anatomical parts. From 40 to 60 weeks all parts contribute about equally to the success of the reaching movement.

9. From 12 to 20 weeks lateral digressions in reaching usually occur throughout the entire course of the approach. From 24 to 36 weeks the largest digressions occur midway of the course and from 40 to 60 weeks the principal digressions which are now relatively small take place either at the start, end, or midway of the reaching movement.

10. The early elimination of median digressions is essential to accurate reaching in order that the hand may not obstruct vision of the pellet.

11 Arm movements which result in under-reaching the pellet are common in the first half year of life.

12. Ulnar flexion of the wrist in pointing, which is an adult characteristic, begins to function after the sixth month.

13 The amount of elbow extension is not an index of its usefulness in reaching.

14. The time consumed in reaching is in no way a measure of the speed or accuracy of arm movement However, infants of 40 to 60 weeks accomplish the reaching act in one-half the time (1 0 to 1 5 seconds) required by younger infants

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Clinic of Child Development
Yale University
New Haven, Connecticut

L'ACQUISITION DE L'HABILETÉ DANS L'ENFANCE

(Résumé)

En contraste avec l'acquisition de l'habileté par les enfants plus âgés et les adultes, l'enfant en bas âge au commencement de cette enquête n'a aucun répertoire de mouvements acquis dont il peut se servir dans le développement de l'habileté Il faut qu'il la construise sur des activités motrices non apprises. Avec ce désavantage, cependant, son habileté à étendre la main à 60 semaines compare favorablement à l'habileté des adultes Dans l'acquisition de l'habileté à étendre la main pendant les 60 premières semaines de la vie les premiers mouvements sont des reflexes ou de simples réponses motrices sensorielles dont les mouvements abducteurs et adducteurs de l'épaule sont les plus prononcés. La prochaine étape est l'époque des mouvements volontaires lentement acquis qui s'étendent des activités grossières a tâtons jusqu'à l'action directe d'étendre la main Dans la dernière étape les mouvements sont principalement automatiques. Comme résultat de l'exercice l'habileté est maintenant une forme motrice de réponse plus ou moins fixe L'habileté à étendre la main se développe des mouvements latéraux du bras, non continus, jusqu'à des mouvements de la main en avant, doucement continus. L'ordre du développement fonctionnel des parties anatomiques composantes est épaules, coude, doigts, tronc et poignet. Les erreurs latérals deviennent plus frequentes jusqu'à 24 semaines et puis deviennent de moins en moins fréquentes jusqu'à 40 semaines; après cet âge-ci ces erreurs sont uniformément petites L'élimination de bonne heure des erreurs vers le plan médian est essentielle à la précision d'étendre la main, pour que la main ne rende pas obscure la vision de l'objet

HALVERSON

DIE ANEIGNUNG VON GESCHICKLICHKEITEN BEI SAUGLINGEN

(Referat)

Der Saugling steht insofern zu alteren Kindern und Erwachsenen in Gegensatz als er, am Anfang der Untersuchung, noch kein Repertoire der angeeigneten Bewegungen besitzt die er beider Entwicklung einer besonderen Geschicklichkeit verwenden konnte Er muss sie auf Basis ungelernter motorischer Tätigkeiten aufbauen Trotz dieses Hindernisses, jedoch, lässt sich seine Geschicklichkeit mit 60 Wochen gut mit der Geschicklichkeit Erwachsener vergleichen. Bei der Aneignung der Geschicklichkeit im Greifen [reaching] wahrend der ersten 60 Lebenswochen sind die ersten Bewegungen Reflexe oder einfache sensoriel-motorische Reaktionen worunter Bewegungen der Schulter nach aussen und nach innen [abductive-deductive movements] am starksten hervorragen Das nachste Stadium ist die Periode der langsam angeeigneten willkurlichen Bewegungen, die sich von ungeschliffenen, tastenden Tätigkeiten bis zum direkten Greifen erstrecken Im endgultigen Stadium sind die Bewegungen grossenteils automatisch In Folge der Übung stellt die in Frage stehende Geschicklichkeit nun eine mehr oder weniger definitiv etablierte motorische Reagierungsweise [pattern of response] dar. Die Geschicklichkeit im Greifen entwickelt sich vom Stadium der unterbrochenen Seitenbewegungen der Arme bis zu glatt verlaufenden, ununterbrochenen Vorwartsbewegungen der Hand Die Reihenfolge der funktionellen Entwicklung der an den Bewegungen teilnehmenden anatomischen Teile ist folgende, Schultern, Ellbogen, Finger, Rumpf, und Handgelenk Seitliche Fehlgriffe [lateral errors] nahmen bis zu 24 Wochen zu und nehmen dann bis zu 40 Wochen allmahlich ab, nach welchem Alter diese Fehlgriffe ubereinstimmend klein sind Die fruhe Ausschaltung der Fehler in der mittleren Gegend [median plane] ist zu genauem Greifen wesentlich, da sonst der zuergreifende Gegenstand durch die Hand teilweise von den Augen verborgen sein kann.

HALVERSON

THE ANIMAL MIND*[1]

From the Animal Laboratory, Department of Psychology, Columbia University

C. J. Warden

Perhaps no problem in the field of psychology has aroused more genuine interest than that regarding the nature of the animal mind. The question was raised in the time of Aristotle—if not long before—and has remained to this day a veritable storm center of controversy. That the question has wide ramifications is shown by the fact that it has been exploited through the ages by men widely divergent in interests, temperament, and training. The philosopher and theologian as well as the naturalist and psychologist have attempted to solve—each in his own way—the ubiquitous riddle of infrahuman mental life and behavior. Broad generalizations have been hazarded by men of no scientific training on the basis of a few insignificant concrete facts. Facts have been neglected outright, or unduly subordinated to theoretical bias, by those conversant with a wider field of knowledge. No problem, indeed, has suffered more from preconceived notions and philosophical prejudices.

In spite of the traditional conceptions that have accumulated around the problem, only a few distinctive solutions have been proposed up to the present. It seems possible, indeed, to include practically every shade of opinion under one or another of three general viewpoints. If we think of these as being represented by as many contending parties, we may speak of them as the *right wing*, the *left wing*, and the *center*. The right wing includes that group, of whatever field and time, which seeks to interpret the mental life of the animal in terms of human consciousness. This position obviously fosters the tendency to humanize the animal and thus to place a high and favorable estimate on infrahuman mental life and behavior. It is often associated with the extensive use of anecdotal evidence, since stories of unusual exploits in animals may seem to indicate that the higher types are "almost human." The right wing was in the ascendency during the anecdotal movement which followed the pro-

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mulgation of Darwin's theory of evolution—to which we shall presently refer.

The left wing party takes the opposite position, the distinction between man and animal being greatly emphasized. The lower animals are regarded as simple machines while the higher animals are conceived of as little more than larger and more complex machines If the mind and behavior of the animal is dragged down to the mechanical level, that of man is often raised to a superhuman level in the same breath. The center party includes all those who refuse to take either of the two extreme positions They reject both anthropomorphic interpretations and narrow mechanistic explanations and consider the controversy between the right and left wing parties as futile if not ridiculous.

The problem itself seems to be ever with us and, if one generation happens to be dominantly right wing, the next is almost certain to turn left wing. This tends to keep the question continually in the field of controversy after the manner of a political or religious issue. Something of the general trend of opinion on this point may be gained by a brief reference to a few outstanding periods of human thought.

We may well begin with Aristotle who was the founder of both biology and psychology. While not as extreme as certain later writers, Aristotle must be regarded as a representative of the right wing party. He held that the higher animals possess knowledge, wisdom, and sagacity differing from that of man only in degree. He went so far as to say that the human child "hardly differs for the time being from an animal." He speaks of a certain species of spider as "preëminently clever and artistic" because of its ability to weave a web of unusually intricate design. He regards the elephant as next to man in intelligence, and insists that "it can be taught a number of tricks, the drift and meaning of which it understands," such, for example, as "kneeling in the presence of the king" Moreover, he ascribed most of the human passions to the higher animals. Craft and treachery, jealousy and self-conceit, nobility and courage, apply to man and animal alike His language seems to imply a certain degree of ethical consciousness in some cases, as, for example, when he speaks of the mare as "sexually wanton," of the partridge as "salacious," and of the crow as "chaste." In brief, his interpretation of the animal mind was naïvely anthropomorphic, and most of his evidence was drawn from anecdotal sources. Much the

same point of view was held by Plutarch and by Pliny the Elder, a few centuries later. The latter reports that an elephant in training for the Roman circus was found practicing his dancing lesson alone at night in the moonlight after being punished severely for doing poorly that day. These writers all seemed to argue that if an animal behaves like a man it must, perforce, also feel and think like a man. This principle, although often disguised by artful language, is basic to the position of the right wing party of whatever age

Perhaps Descartes may be regarded as the first outstanding leader of the left wing party. As everyone knows, this seventeenth century philosopher formulated the extreme mind-body dualism with which philosophy is still more or less afflicted. Since he regarded mind as essentially an eternal substance he was forced to deny mind to infrahuman organisms or to admit that they, as well as man, must be immortal. He chose the former alternative even as Liebnitz somewhat later chose the latter. Descartes held that the animal had no mental powers but was merely a sort of natural machine. He was thus forced to deny that animals possessed sensations and feelings of any kind, although he appears, in a later phase, to have admitted the possibility of vague sensations and desires in the higher animals In his first period, he boldly declared that if a machine could be made exactly resembling in organs and outward form an *ape, or other animal, its activities could not be distinguished by man* from a genuine living individual of the given type. Malebranche, a follower of Descartes, asserted that even such animals as dogs and cats "eat without pleasure, cry without pain, grow without knowing it, desire nothing, know nothing . . ." They are able, he avers, to make apparently intelligent adjustments only "because God, in making them for self-preservation, has constituted their bodies in such a way that they withdraw organically and without knowing it from all that can destroy them and that they seem to fear"

The Cartesian doctrine, although extremely mechanistic, engendered no conflict with the orthodox theology of the times, since it left the human soul intact and upheld the primacy of man Descartes himself declared that "after the error of atheism, there is none which leads weak minds further astray from the path of virtue than the idea that the minds of animals resemble our own, and therefore that we have no greater right to a future life than have gnats and ants . " The very language of the arguments used

shows clearly that the left wing position was formulated, not primarily because the facts seemed to require it, but in order to uphold certain philosophical and theological opinions.

Thus we see that the issue between the two opposing camps was very clearly drawn with the rise of Cartesianism in the seventeenth century. On the one hand we have anthropomorphic analogy, often carried to a ridiculous extreme, and on the other a narrow mechanistic interpretation of animal activities that belies the simple biological and psychological facts of everyday experience The right wing party drew its chief inspiration from Aristotle and later Greek writers It rested primarily upon the naturalism of the Greek mind. For, had not Aristotle taught that man was merely the highest animal? Had he not held that man belonged within the world of nature rather than outside or above it? As a matter of fact the naturalists as a class have always been generous in their estimates of the animal mind The right wing party has always been composed to a large extent of genuine lovers of nature, with perhaps something of the sentimentalist in their make-up.

The ungenerous views of the left wing party are usually supposed to represent the hard and cold logic of the scientific mind. Doubtless this general position does rest in part upon a proper critical attitude but often with a rigor that seems much too zealous to be strictly scientific. After all, Aristotle was much more of a scientist than Descartes from every point of view. As a matter of fact, the left wing party drew much of its inspiration from the rising tide of mechanistic philosophy and science of the seventeenth and eighteenth centuries Even before this it was fostered by the so-called rational psychology of scholasticism which insisted, for theological reasons, upon the primacy of man. Man was regarded as a rational being capable of moral responsibility, while the animals were merely driven about by uncontrollable instinctive impulses. The left wing position cannot be considered as a purely scientific reaction against the naiveté of the sympathetic naturalist. It has too many historic connections with philosophic and religious movements to press the claim of a scientific origin. In fact, its scientific connections are no more direct than are those of the right wing party.

The controversy over the nature of the animal mind was raised anew by the announcement of the theory of evolution by Darwin in 1858. It is obvious that a thoroughgoing theory of evolution must include the mind of the infrahuman organisms and man him-

self in a continuous series. Even such ardent followers of Darwin as Wallace and Huxley hesitated to believe that man's mental, moral, and spiritual faculties had been derived from their rudiments in the higher animals. The doctrine of mental evolution as inclusive of the *genus homo* thus became the storm center of the whole controversy over Darwinism. The notion that mind as well as bodily structure and function had developed from natural causes amounted to a denial of the primacy of man in any but a purely quantitative and naturalistic sense. The mind-body dichotomy was rather clear-cut in the public mind because of the fact that the philosophic thought of the time was more or less permeated by Cartesian dualism. The bitter attacks of the theologians and religious leaders, who thought they saw in the new doctrine the collapse of moral culture and religious faith, added tenseness to the situation. It was this stubborn refusal on all sides to accept the fact of mental evolution, in so far as it applied to man, that was in a large part responsible for the anecdotal movement in comparative psychology.

The anecdotalists confined their efforts almost wholly to the collection of stories, sometimes of doubtful veracity, emphasizing the human-like behavior of the higher animals. The lower organisms were of slight interest since the traditional science and theology had placed the impassable gap to mental continuity between the higher animals and man. The problem thus practically narrowed down to showing that the higher animals possessed a rudimentary intelligence, out of which the more diversified human mental life might conceivably have developed. The need of the hour, if the doctrine of mental continuity and evolution were to survive, was for a mass of concrete evidence, tending to show the beginnings of rational, emotional, social, and moral capacities in the higher animals. In the absence of first-hand observations, appeal was had to the anecdote, which had always been in good repute in illustrating unusual conduct in animals in natural history collections.

Darwin himself made considerable use of anecdote and few if any of the writers on natural history of this period avoided it altogether. Scores of anecdotal collections appeared in which the tendency to humanize and eulogize the mental powers of higher animals reached the utter ridiculous. These collections often included material from Aristotle, Pliny, Plutarch, and other of the ancients in addition to that gathered from contemporary story-mongers. In many cases the anecdotes were taken from unreliable sources or were

mere hearsay, and in all cases the moral to the story was that the animal was "almost human" if not actually so.

The essential argument in much of the anecdotal material is difficult to follow on account of the loose manner in which fact and fancy are thrown together. In general, however, the anecdotes may be grouped so as to constitute the following three main lines of so-called evidence in support of the contention that the higher animals possess a rudimentary human mind: (1) anecdotes purporting to show some measure of reasoning ability, (2) anecdotes supposedly illustrating social behavior of a high order, and (3) anecdotes in which characteristic human emotions indicative of sympathy, shame, deceit, courage, timidity, suspicion, jealousy, curiosity, emulation, sense of justice, sense of humor, etc., are apparently exhibited. Cunning and ingenuity in the natural environment such as that shown by the beaver in building its dam at strategic positions, or in domestic and captive animals in outwitting man, or learning tricks with or without tuition would properly belong in Class I. Imitation, purposive cooperation, intercommunication of ideas and plans of action by means of signs or sounds that serve the language function would fall in Class II The fallacy of anthropomorphic analogy reached its worst in the fanciful interpretations of Class III, the underlying assumption seeming to be that if the animal acts like man it also feels and moralizes like man.

The following arguments may be urged against the anecdote as a source of scientific information regarding the mental life and behavior of animals: (1) that the observer is likely to be untrained and unable to give an accurate account of the happening, even if his intentions are of the best; (2) that the interpretative elements are likely to be confused in the report with the factual, making it impossible for the scientist later to separate the two; (3) that the happening even when adequately reported is usually an incident cut off from the essential genetic antecedents (both individual and phyletic) which would explain it and give it proper significance; (4) that the happening, in the nature of the case, represents highly selected and atypical behavior that can have little or no statistical validity; (5) that even if the tendency of mankind to humanize the animal—whether in a scientific or literary mood—is restrained, errors of memory and of transmission (if verbal) are likely to enter, and (6) there is the difficulty of selecting reliable, authentic material from the various available sources. It is evident that the method,

even when guardedly employed, which usually is not the case, hardly deserves to be considered scientific in the strict sense.

The anecdotal collections were widely read and the popular imagination was deeply stirred. In fact, the wide appeal to the anecdote by the protagonists of the doctrine of mental evolution was not altogether without value as a reaction against the older instinct-reason antithesis. Just as philosopher and theologian had previously twisted the facts in order to create an insuperable gulf between the mind of man and animal, so now by the opposite bias that gulf was not only bridged, but the difference between the mind of man and animal unduly minimized—owing in large part to the controversial temper of the times The general acceptance of the view of Darwin that the entire mental life of man must be included in the general scheme of mental evolution made the appeal to the anecdote no longer necessary or excusable.

As might have been expected the last decade of the nineteenth century witnessed a sharp reaction against the anecdotalism and anthropomorphism of the right wing party. The outstanding leader in the new left wing movement was Jacques Loeb whose interest in the animal mind grew out of his studies in comparative physiology The physiologists of the day were greatly concerned with the direct reactions of simple plants and animals to external stimuli The responses of simple forms to light, gravity, and other environmental energies *are often uniform and predictable and are commonly known as* tropisms. Taking his cue from this field of study, Loeb sought to extend the concept of tropistic reactions to the behavior of the invertebrates as a group Since tropistic behavior was then regarded as physico-chemical and unconscious, this meant the denial of mind to all but the higher animals. Still later, he further extended his tropism theory to cover many of the so-called instinctive activities of the higher animals and of man

The tropism theory of Loeb was little short of a complete return to the mechanistic position of Descartes, and came as a fatal shock to the post-Darwinian humanizers. This bold attempt to analyze the behavior of lower organisms by physiological methods and to explain it in purely objective terms was little short of heroic in view of the anthropomorphic babblings of the times It was, indeed, a far cry from the notion of Romanes that the insect flies into the candle flame out of innate curiosity to the contention of Loeb that it is forced to do so in a very literal sense when presented with the appro-

priate external stimulus. Among other criticisms of Loeb's theory the satirical attack of Claparède is interesting. He pointed out that an observer from a distant planet might well suppose that human activity was also largely tropistic and lead such a one to speak of the doctor-tropism, corpse-tropism, food-tropism, etc, of mankind.

The tropism theory of Loeb offered a distinct challenge to the old school that had taken more or less for granted the supposition that even the lowest organisms possessed some form of psychic life. Loeb held the elemental component of all psychic life to be "the activity of the associative memory" and maintained that, unless an organism could be shown to possess the capacity to form associations, its behavior must be considered unconscious. "Our criterion," he says, "puts an end to metaphysical ideas that all matter, and hence the whole animal world, possesses consciousness." The logical conclusion is that tropistic and instinctive behavior is wholly unconscious and that such organisms as are incapable of definitely profiting by experience are nothing more or less than reflex machines

This general point of view was accepted by Beer, Bethe, and von Uexkull, Nuel, Ziegler, and others, and by Bohn in a somewhat modified form On the basis of this criterion, this group contended that most probably the invertebrates generally were devoid of consciousness. At any rate, an adequate account of their behavior could be given in physico-chemical and behavior terms and without reference to any possible psychic life. In much the same vein, Titchener denied consciousness to plants, although accepting the current view of the right wing that all animal forms are conscious in some sense Bethe's contention that ants, bees, and other insects are mere reflex machines stirred up no little controversy among the entomologists. Wasmann, although denying rational intelligence to even the higher vertebrates, insisted on assigning some sort of psychic life to insects, while Buttel-Reepen, and especially Forel, held that insects give evidence of a relatively complex mental life. Forel went so far as to maintain that their behavior involves memory, associations of sensory images, perceptions, attention, habit formation, and simple powers of inference from analogy

The long controversy between the right and left wing parties led in time to concessions from both sides Perhaps Lloyd Morgan best represents the tradition of the post-Darwinian school, which insisted on the validity of anthropomorphic interpretations of the animal mind He was among the first, however, to turn against the

extreme humanizing tendency which had been carried to such absurd lengths by the earlier anecdotalists. He began his attack by deploring the use of anecdote and insisting that observation and experiment were the proper tools of the comparative psychologist as of other scientists. He recognized the fact that man naturally tends to be anthropomorphic in his attempts to interpret the behavior of infrahuman types, and hence must ever be on his guard in this respect. He embodied this principle in the well-known canon of Morgan which runs as follows: "In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale." This is merely the law of parsimony—the critical canon of general science—applied to the field of comparative psychology. While this seemed at the time to be a radical position, it has come to be generally accepted by the right wing party. The basic principle of the right wing—that anthropomorphic analogy is valid procedure—was by no means given up. The canon merely insisted that a certain amount of restraint should now be exercised in attempting to infer consciousness from behavior. While the measure was intended to be merely precautionary, nevertheless it amounted to a large concession to the left wing party by effectively reducing the status of the animal mind.

The left wing party under the leadership of Loeb had rested their case upon the criterion that consciousness was coextensive with the capacity to form associations, or habits. They held that if an animal could profit by individual experience it must be conscious in some sense. In general, the invertebrates were regarded as being without psychic life because it was assumed, on the basis of the work of Bethe and others, that they were incapable of even simple habit formation. Their activities were thought to be tropistic and reflex throughout—and both tropisms and reflexes were defined as innate and unchangeable responses. This attempt of Loeb and other mechanists to restrict consciousness to the higher animals was brought to naught by later experimental results showing that even the lowest of the invertebrates can learn under appropriate conditions. The experimental facts were brought out by the early work of Jennings, Piéron, Yerkes, and others. In general, these investigators found that the behavior of lower organisms is not only variable but modifiable within limits. Such animals profit by experience much as do higher forms in ordinary trial-and-error situations. If

habit formation is to be taken as the crucial evidence for consciousness, then we shall have to admit that all living organisms are conscious, since all of them can learn something. While the left wing party has never openly confessed defeat, this does not alter the fact that they have been successfully put to rout, since their criterion itself is meaningless with our present knowledge An honest logic would force them either to set up a new criterion or to admit that so far as they know all animals may be conscious in some sense.

It should be clear that the right and left wing parties are much closer together now than formerly The difference between the views of Lloyd Morgan and his school and the views of Loeb and his school are, in reality, not very great. Surely the distinction is much less marked than that between Aristotle and the post-Darwinians, on the one hand, and Descartes and his followers, on the other The controversy is no longer between naive anthropomorphism and an out-and-out denial of mental life to all infrahuman organisms The right wing party has committed itself to the principle of critical anthropomorphism in accepting the canon of Morgan, while Loeb and his followers have never denied consciousness to the higher animals after the manner of Descartes In a sense, the tropism theory of Loeb was merely an application of the canon of Morgan—albeit somewhat strictly—to the lower organisms. For, if the behavior of these forms can be explained without an appeal to a psychic factor, then, according to the canon of Morgan, such a factor should not be brought in The main point of difference between these two schools, of a generation ago, was as to the extent of a mental life of some sort among living organisms Loeb held that the psychic factor made its appearance only in the vertebrate stem, while Morgan inclined to the opinion that all living organisms possessed a low form of consciousness which he termed sentience. Morgan denied reasoning ability to all the higher animals, and most probably did not rate their mental life much above the level to which Loeb would have been willing to subscribe It is small wonder, then, that the controversy between the right and left wing parties gradually died out.

This turn of events gave the center, or moderate, party an opportunity to come to the fore—perhaps for the first time in history. This group began by deploring the whole controversy concerning the presence and nature of the animal mind as futile and ridiculous They stressed the fact that any attempt to interpret the subjective

life of the animal was fallacious anthropomorphism. The inner life of the animal, if such they have, is forever hidden from man for the simple reason that we can never gain any first-hand knowledge of it. It may well be that a dog is conscious, but how can the psychologist, who has never been a dog, possess any insight into the mental states of his pet? As a matter of fact, it is difficult enough for one person to understand the experience of another by means of language and emotional expression. This difficulty is greatly increased when the other person is an infant below the language level or an insane person possessed of an inner life that must differ from our own. The difficulty becomes insuperable, however, when the organism is an animal whose experience must be essentially different from that of man and who has no language by which to communicate to us anything whatsoever concerning the nature of that difference. Moreover, the question as to whether an animal is conscious *or not has little or no significance if we are unable to determine* anything regarding the nature of its mind. The attitude of the central group is thus one of frank agnosticism. It should be borne in mind, however, that to deny that we can ever know anything about the mental life of infrahuman organisms is very far from denying that they possess a psychic life of some sort.

The adoption of this point of view meant that a new and wholly objective approach must be made in the analysis of the activities of the organism. In the new system of analysis, such distinctions as mind-body, subjective-objective, conscious-unconscious, and the like are not only irrelevant but essentially meaningless. The behavior of the organism as a whole is regarded as a self-explanatory system when the proper modes of analysis are employed. The objectivist may speak of intelligent and non-intelligent behavior but never of conscious and unconscious behavior in dealing with the animal subject. Whether behavior is intelligent or not is a matter that can be checked objectively, and to say that behavior is intelligent is by no means to say that it is conscious. When behavior involves an adjustment to environment in a manner which successfully meets the needs of the organism it may be regarded as intelligent. As thus broadly applied the term covers much behavior which, even in man, is usually regarded as unconscious. The center party, which is dominant at the present time, thus claims to know nothing of the animal mind except the bare fact that they can never know anything about it. Neither its existence nor its non-existence can

be proven in specific cases, nor in general. The term *mind,* and with it all subjective implications, has been dropped from the universe of discourse of modern comparative psychology As everyone knows, Dr. Watson would drop it as well from the field of human psychology, but I shall not enter into any discussion of this point since it does not directly bear upon my topic.

The development of the objective viewpoint in the animal field forms a very interesting chapter in the history of psychology. In the brief time at my command I shall be able to mention only a few of the more immediate influences that led to its emergence Perhaps the most important single factor was the vigorous growth of experimental work in the animal field. Laboratory studies of animal behavior began with Lubbock and Lloyd Morgan about 1890 and rose to prominence, especially in America, around the turn of the century under the leadership of Jennings, Thorndike, Kline and Small, Parker, Yerkes, and many others The experimentalists became more and more interested in observing and reporting behavior and showed a constantly decreasing interest in subjective speculations of the interpretative sort. The famous attack of James on the concept of consciousness as an entity and the assertion of Cattell that even the human psychologist might deal with the purely objective phases of behavior without recourse to introspection and subjective interpretation doubtless were also effective. It was the general attack of Watson on the introspective method and mentalistic concepts, however, which initiated the objective movement as such in the animal field His position that any attempt to deal with the problem of the presence and nature of consciousness was sheer speculation became the guiding principle of the center party.

Perhaps there are some who consider the present objective movement in comparative psychology as merely a continuation of the left wing party. It should be clear, however, from what has gone before that this is not the case. Loeb did not deny the validity of inferring the presence of psychic life in infrahuman organisms from their behavior. As a matter of fact, he explicitly insisted that such inferences were possible and formulated a criterion for determining the matter, i.e., the ability to profit by experience. Furthermore, he denied the presence of consciousness in the invertebrates on the basis of his criterion, and admitted that the vertebrates possessed consciousness. Such a position is far from a frank agnosticism regarding the whole issue. Loeb did not deny the mind-body dichotomy

although he was seeking to free the study of behavior from the dominance of metaphysics. Furthermore, Loeb's viewpoint is shot through and through with a narrow mechanistic philosophy which is thoroughly obnoxious to the comparative psychologists of today. While a certain overemphasis has been placed upon narrow behavioristic and reflexological formulations by some comparative psychologists, the dominant trend in this field at present is toward a broad organismic conception of behavior

Although the animal psychologist of today must disclaim any knowledge of the animal mind, he harbors great aspirations for the future of his science. For he finds that it is possible to understand behavior in terms of objective categories without raising the problem of the possible conscious concomitants of the various activities involved This may be accomplished by a systematic analysis of the organism in terms of typical stimulation-response processes. The task is by no means simple even in the case of simple organisms and it becomes increasingly difficult as we approach the human level in the phylogenetic scale The outstanding handicap, perhaps, in behavioral study is our inability to give the animal verbal directions concerning what is to be required of him in test situations. But this is compensated for, to some extent at least, by the fact that we can control our test conditions much more rigorously than is usually possible in the human field. Moreover, we can employ operative and other extreme measures which are manifestly out of the question in dealing with human material.

Most of the problems of the traditional human psychology—except those dealing directly with subjective experience—can be included in the general program of behavioral analysis. We may regard the larger field of animal behavior as comprising two main subfields, the one concerned with sensory capacities and the other with reactive capacities. Methods for the objective study of sensory capacities have become fairly well standardized in recent years Such indices as sensory acuity, discrimination limens of various sorts, range of sensitivity, and the like can now be determined, by long and painstaking experimental procedures, within each of the great stimulus domains. Special problems in the visual field relating to size, form, and pattern discrimination, illusions, color vision, etc , can also be attacked Such intricate problems as pitch, loudness, and timbre discrimination, sound localization, and the like in the field of audition are now open to intensive study.

In the general subfield of the reactive capacities, we must resort very largely to field observation in investigating typical behavior associated with the securing of food, protection, primary and secondary sex activities, and natural biotic community life. In the main our laboratory work here centers around the three following topics: (1) learning and retention ability, (2) motivation factors underlying behavior, and (3) general level of intelligence. Each of these classes of behavior may be approached by the genetic method, in which the developmental aspect is emphasized, as well as by the usual cross-section method. Finally, the program of behavioral analysis includes the intercomparison of types and individuals in terms of common indices of behavior in so far as these may be secured.

It is interesting to note in this connection that the vigorous growth of animal experimentation has greatly influenced the trend of development in human psychology The findings in the animal field cannot, as a rule, be carried over directly to the human field. But the extension of supposedly human problems to infrahuman organisms has often upset subjective explanations that had been long regarded as well established. It has been necessary, on account of the findings in the animal field, for the human psychologist to eliminate certain explanations altogether and to shift the point of emphasis with respect to others. In general, we may say that animal psychology has operated as a corrective to human psychology at nearly every turn. The newer comparative psychology is essentially biological in the broader sense, and human psychology is gradually coming to adjust itself to this natural and fruitful point of view We hear less of ideas and intellection in human psychology today and more of intelligence than formerly, the latter being regarded as the activity of the organism as a whole rather than as a faculty or activity of the mind The subjective studies of a generation ago, which were of no interest outside of academic circles, now lie dust-covered and forgotten. The human psychologist has at last become interested in behavior rather than mental states and doubtless the movement in the animal field is largely responsible for this.

The notion seems to persist in some quarters that the primary aim of animal studies is to make discoveries that will throw some light upon human behavior problems. But such a narrow view of animal psychology has long since been outgrown. The animal psychologist of today seeks to find an intrinsic interest in the behavior of each type studied. The broader aim of the science is to make all possible

intercomparisons of behavior and not merely to relate the behavior of the animal to that of man The difference in behavior between the amoeba and the earthworm may be of more consequence in the systematic development of the science than the difference between chimpanzee and man In brief, the comparative psychologist is interested in behavior at all levels of complexity. He hopes in time to build up a science of evolutionary psychology as broad in scope as the sister sciences of comparative morphology and comparative physiology At the present time neither of the latter disciplines is dominated by human material. Animal psychology must seek to attain a like degree of independence if it is ever to become a well-rounded and systematic science, and thus achieve its natural destiny.

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Columbia University
New York City

L'ESPRIT DES ANIMAUX

(Résumé)

C'est un résumé historique et critique du développement de l'opinion dans le domaine de la psychologie comparative L'auteur reconnaît trois principales écoles de pensée L'aile droite, l'aile gauche, et le centre Le parti de l'aile droite est partisan de l'anthropomorphisme dans l'interprétation des activités des animaux et tendent donc a humaniser le comportement Le parti de l'aile gauche se compose de mecanistes extrêmes tels que Descartes et Loeb Le parti du centre ne nie pas la présence de l'esprit chez les animaux mais pense qu'il est impossible de le connaître, ou directement ou par inférence Se dernier groupe, maintenant le plus fort, se compose d'organicistes plutôt que de mécanistes puisqu'il appuie sur l'importance de la complexité de l'organisation des systèmes vivants Il etudie le comportement au moyen de méthodes experimentales quand possible et cherche a expliquer le comportement des animaux en termes de catégories objectives plutôt que mentalistes Ce mouvement est mieux représente dans la psy-

chologie comparative de l'Amérique, surtout dans ses développements les plus récents. Dans le laboratoire américain, on a inventé des méthodes pour la mesure precise des capacités sensorielles dans des conditions contrôlées comme il le faut Tout le domaine de la discrimination sensorielle est maintenant ouvert à l'analyse expérimentale L'étude des capacités du comportement devient aussi de plus en plus quantitative en precise Cela est surtout vrai dans les domaines des mobiles, de l'apprentissage, et de l'intelligence L'emploi réussi des méthodes objectives dans le domaine de l'étude des animaux a tendu à rendre la psychologie humaine moins subjective et plus scientifique.

WARDEN

DER GEIST DES TIERES

(Referat)

Der Verfasser bietet uns hier einen Bericht uber die Entwicklung der wissenschaftlichen Meinung im Bereich der vergleichenden Psychologie Er erkennt drei Hauptschulen der Meinung an einen rechten Flugel, einen linken Flugel, und ein Zentrum Die Partei des Rechten beharren auf den Anthropomorphismus bei der Deutung der Tatigkeiten von Tieren und ist also zur Vermenschlichung [humanizing] des Betragens geneigt Die Partei des linken Flugels besteht aus extremen Mechanisten, so wie sie Descartes und Loeb darstellen Die Partei des Zentrums verneint nicht die Existenz des Geistes bei Tieren, behaupten aber, dass dieser Geist [mind] sowohl direkt wie durch Folgerung unkennbar ist. Diese letztere Gruppe, die jetzt die Oberhand hat, sind eher Organisisten wie Mekanisten, da sie die Wichtigkeit der Kompliziertheit der Organisation bei lebenden Systemen betonen. Sie untersuchen des Benehmen wo moglich mit experimentellen Verfahren und versuchen, dass Benehmen der Tiere unter Verwendung eher objektiver als innerlich-geistiger [mentalistic] Begriffe zu erklaren. Diese Bewegung wird am Besten durch die vergleichende Psychologie der Vereinigten Staaten, besonders in deren neulicheren Entwicklungen, am Besten vertreten. Im amerikanischen Laboratorium sind Methoden erfunden worden fur die genaue Bestimmung der Sinnesfahigkeiten [sensory capacities] unter richtig kontrollierten Bedingungen. Das ganze Bereich der sensoriellen Unterscheidungsfahigkeit [sensory discrimination] ist nun der experimentellen Analyse zuganglich. Die Untersuchung der Fahigkeit zu Tatigkeiten [behavioral capacities] gestaltet sich ebenfalls immer mehr quantitativ und genau Diese Behauptung bezieht sich besonders auf die Bereiche der Motivierung, des Lernens, und der Intelligenz Die erfolgreiche Verwendung der objektiven Methoden im Bereich der Tierpsychologie ist danach gerichtet, dazu, auch die Menschenpsychologie weniger subjektiv und mehr wissenschaftlich zu gestalten

WARDEN

ADOLESCENT INTERESTS AND ABILITIES*

From The Lincoln School of Teachers College, Columbia University

GERTRUDE HILDRETH

Personality and interest questionnaires serve two major purposes in the high school. They contribute, on the one hand, to pupil guidance and adjustment activities, and, on the other, to research in the psychology of adolescence. For both purposes a questionnaire or check list is more satisfactory than opinion based on cursory observation of pupils or uncontrolled interviews in which emotional factors often invalidate evidence. Nevertheless, for both guidance and research functions the questionnaire has certain limitations which have previously been pointed out and will be reiterated here. Since the research and service functions constitute two separate topics, the present report will be confined to a summary of questionnaire results as a contribution to adolescent psychology.

A number of studies to which references are given in the bibliography have previously been made of the relation between interests and abilities of school children. Most of these investigations reveal some degree of positive correlation between mental maturity, intelligence, and interests of various types—sports, school subjects, reading, vocations, and other activities. The essentially new feature of the present report is the investigation of the interests and abilities of high-school pupils of two distinctly different social and economic levels. One group consists of pupils from a private school, enrolling pupils from homes of culture and superior social and economic status. Objective evidence that the majority of homes from which these pupils come are superior to average homes has been obtained through the use of the Sims score card. Practically all of the pupils live in New York City or in adjacent suburban communities. The second group of pupils live in an industrial city not far distant. The homes from which these children come are typical of industrial communities the country over, though the proximity to New York gives this community the advantage in cultural opportunity. In both

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groups there are pupils whose parents are foreign-born, but in the private-school group such parents are scientists, business executives, or professional people; whereas in the public-school group the foreign-born parents are primarily industrial workers

To these two groups of pupils a series of experimental scholastic aptitude and achievement tests and a check list of activities and interests were given under uniform conditions. Identical directions were used in examining in both types of schools. The experimental tests consisted of five parts:

1. *Information* Illustration (*a*) The kangaroo is a native of—(1) North America (2) Africa (3) The West Indies (4) Australia (5) Southern Europe (*b*) Asbestos comes from—(1) trees (2) mines (3) oceans (4) cotton (5) skins.

2. *Vocabulary.* Pupils were asked to write the definitions of fifty words of gradually increasing difficulty, illustrated in sentences: To ABANDON a friend. . . . .. A DENIZEN of the town . . . .

3 *Mathematical Reasoning.* This test consisted of sixteen graded reasoning problems Illustrations: (*a*) If a recipe calls for 2½ pints of milk for 6 people, how many pints are needed for 9 people? (*b*) The temperature, *f*, on a Fahrenheit thermometer equals 9/5 the temperature on a centigrade, *c*, plus 32. Complete this formula: $f=($ $)$ ....

4 *Completions* Twenty-two problems of increasing difficulty. Illustrations. (*a*) Children go to. ....to learn to.. . and write (*b*) The expansion and.... ... ...of metals is.. .. . .changes in .. ..

5. *Reasoning.* A series of 18 problems containing number series completion, analogies, and syllogisms. Scores on these tests were given the following weighting: information, ½; mathematics, 2; completions, ½ Total scores were computed using these weights

A copy of the check list used for obtaining information about activities and interests is found on the following pages. Items to be checked were grouped in the following categories: I. Recreational activities. (*a*) Games. (*b*) Other activities. II. Preferred associates. III School subjects. IV. School preferred after high school. V Reading. (*a*) Books read during the past year or two. (*b*) The types of books liked best. (*c*) Magazines. VI. Vocational preferences.

NAME . AGE GRADE DATE . .

SCHOOL .. . . BOY ...GIRL .

CHECK LIST

I Recreational

A Games—Write the numbers 1, 2, and 3 before the games you enjoy the most, indicating your first choice as No 1.

Anagrams
Archery
Authors
Backgammon
Baseball
Basketball
Billiards
Bowling
Boxing
Bridge
Checkers
Chess
Crap games
Crokinole
Croquet
Cross-word puzzles
Dominoes
Football
Golf
Handball
Hockey
Mah Jong
Marbles
Pingpong
Poker
Polo
Pool
Quoits
Soccer
Squash
Tennis
Track
Volley ball
Wrestling

Others

B Other Activities—Write the numbers 1 to 5 before the activities you most enjoy, indicating your first choice as No 1

Watching athletic events
Looking up information
Gymnasium apparatus work
Taking care of children
Taking care of animals
Belonging to social clubs
Playing Victrola or radio
Playing piano
Playing other musical instruments
Going to Sunday School
Going to church
Writing poems or stories or for magazines
Collecting, e g, stamps
Entertaining company
Speaking to an audience
Drawing or painting
Working around machinery
Dining in unusual places
Embroidery, fancy work
Knitting or crocheting
Working with tools
Doing science experiments
Mountain climbing
Driving an automobile
Flying an airplane
Visiting museums
Inventing things
Day-dreaming
Ice skating
Roller skating
Horseback riding
Attending movies
Attending plays
Attending concerts
Attending operas
Bicycle riding
Social dancing
Folk dancing
Going to parties
Attending lectures
Having "dates"
Building things
Repairing things
Running races
Taking pictures
Making dresses
House work
Giving parties
Composing music
Trap shooting
Studying lessons
Running a club
Playing charades
Amateur dramatics
Farm activities
Talking to people
Giving plays
Others
Racing
Picnics
Fishing
Traveling
Camping
Hiking
Walking
Hunting
Boating
Canoeing
Yachting
Cooking
Reading
Tobogganing
Gardening
Exploring
Singing
Skiing
Snowshoeing
Swimming

II. Preferred Associates

A Checked associates you prefer

1 Older boys	4 Older girls	7 Mother
2 Younger boys	5 Younger girls	8 Father
3 Boys your own age	6 Girls your own age	9 Other adults
		10 No companions

III. School Subjects

A Write the numbers 1, 2, and 3 before the subjects you like best, of those you have taken in high school, indicating your first choice as No. 1

Algebra	General science	Physics
American history	Geometry	Physiology or hygiene
Ancient history	Geography	Shop work
Biology	German	Shorthand
Bookkeeping	Greek	Social studies
Botany	Household arts, cooking, etc	Spanish
Chemistry	Industrial arts	Trigonometry
Civics	Italian	Typewriting
Costume design	Latin	World history
Dramatics	Mechanical drawing	Zoology
Economics	Modern history	
English	Music—singing	Others.
European history	Nature study	
Fine arts, drawing, painting	Orchestra	
French	Physical education	

IV Colleges

A Check the type of school you would like to attend after high school, or when you are qualified Write in the name of the school if you know it

State university
State agricultural school
State engineering school
Normal school
Teachers college
Art school
Trade school
Large coeducational college
Small coeducational college
Small women's college
Small men's college
Large men's college or university
Large women's college or university
Kindergarten training school
School of commerce or business school
Music school
Finishing school
Boarding school
Dramatic school
Theological seminary
Medical school

Law school
Dental school
Junior college
No school

V Reading

A Books

1 Write below the names of the four books you liked best of those you have read during the past year or two

2 Underline the type of book you like best in general

Directions for doing or making things	
Encyclopedias and books of knowledge	Short stories
Stories of home and school life	Philosophy
Nature and animal stories	Biography
Novels and love stories	Classics
Folk tales and legends	History
Historical novels	Science
Adventure stories	Travel
Nonsense stories	Poetry
Mystery stories	Plays
Others	Arts

B Magazines

1 Write the numbers 1, 2, and 3 before the magazines you most enjoy reading, indicating your first choice as No 1

American	*Etude*	*Movie Romances*
American Boy	*Everybody's*	*Musical America*
American Cookery	*Field and Stream*	*Musician*
American Girl	*Forum*	*National Geographic*
American Home	*Girl Scouts*	*New Republic*
American Mercury	*Golden Book*	*New Yorker*
Arts and Decoration	*Good Housekeeping*	*North American Review*
Asia	*Harper's Bazaar*	*Open Road*
Atlantic Monthly	*House Beautiful*	*Outlook*
Bookman	*Illustrated London News*	*Photoplay*
Books	*Independent*	*Physical Culture*
Boy Mechanic	*Industrial Arts Magazine*	*Pictorial Review*
Boy's Life	*Judge*	*Poetry*
Century	*Ladies Home Journal*	*Popular Mechanics*
Christian Herald	*Liberty*	*Popular Science*
College Humor	*Life*	*Psychology*
Collier's	*Literary Digest*	*Radio News*
Cosmopolitan	*Manchester Guardian*	*Readers' Digest*
Country Life	*Mentor'*	*Red Book*
Current History	*Modern Priscilla*	*Review of Reviews*
Current Opinion	*Motion Picture Classics*	*Romance*
Delineator		*Saturday Evening Post*
Detective Stories		

Saturday Review of Literature
School and Society
Scientific American
Screenland
Scribner's
Spur
St Nicholas
Time
Theatre Guild Magazine
Theatre Magazine
Travel
True Stories
Vanity Fair
Vogue
Woman's Home Companion
Yachting
Yale Review
Youth's Companion
Others.

VI. Vocational

A. Check the name of three occupations you think you would like best, numbering your preferences in order No. 1 first choice.

Accountant
Acrobat
Actor
Architect
Army or navy officer
Artist
Athletic director
Automobile racer
Automobile repairman
Automobile salesman
Aviator
Baker
Banker
Bank teller
Barber
Baseball player
Bookkeeper
Boxer
Broker
Building contractor
Business executive
Butcher
Buyer of merchandise
Carpenter
Cartoonist
Cashier in bank
Chauffeur
Chemist
Civil engineer
Civil service employee
Clerk
College professor
Comedian
Conductor or motorman
Consul
Costume designer
Dairyman
Dancer
Decorator
Deliveryman
Dentist
Detective
Dietitian
Doctor
Dressmaker
Editor
Electrical engineer
Electrician
Explorer
Factory worker
Factory manager
Farmer
Fireman or brakeman
Fisherman
Florist
Football coach
Forest ranger
Governess
Grocer
Homemaker
Housekeeper
House painter
Hotel keeper or manager
Interior decorator
Interpreter
Inventor
Janitor
Jazz orchestra player
Jeweler
Journalist
Laboratory technician
Landscape gardener
Lawyer or judge
Librarian
Life guard
Life insurance salesman
Locomotive engineer
Machinist
Magazine illustrator
Magazine writer
Manufacturer
Mechanical engineer
Merchant
Milliner
Mining superintendent
Musician
Music teacher
Novelist
Nurse
Office clerk
Office manager
Orator or lecturer
Orchestra conductor
Pharmacist
Photographer
Playground director
Playwriter
Policeman
Politician
Preacher or priest
Plumber
Printer
Private secretary
Psychologist
Publicity manager
Railway conductor
Rancher
Real estate salesman
Reporter
Retailer
Sales clerk
Sales manager
School administrator
School teacher
Scientific research worker
Secret service man
Ship officer
Shop foreman
Social worker
Soldier or sailor
Stage manager
Statesman
Stenographer or typist
Statistician
Stock breeder
Surgeon
Surveyor

Switchboard operator	Truck driver	Worker in Y M or Y. W C A
Tailor	Undertaker	Others·
Teamster	Waiter or waitress	
Toolmaker	Wholesaler	
Traveling salesman	Wireless operator	

Pupils were advised not to confine their choices to the items given in the check list but to supply missing items if those constituted actual preferences For example, if the occupation they wished to enter was not included in the list, pupils were asked to write the name of the occupation at the end of the list and to check it. The analysis of results indicates that private-school pupils were less apt than public-school pupils to restrict their choices to the conventional prepared list. Occasionally instructions required the checking of several items in order of preference. This constituted a difficulty for many of the children who stated that they were unable to distinguish between the order of their choices, although they had no difficulty in selecting from the list the activities and interests they preferred. For this reason in some of the tabulations of responses no distinction was made between choices of first, second, third, or other order

Both the check list and scholastic tests were given to all ninth- and twelfth-grade pupils in the two schools These grades correspond to freshman and senior high-school years The number of pupils, the sex distribution, and the median chronological age of each group are given in Table 1.

The chief limitation to the questionnaire as a research instrument is the difficulty of determining the reliability of pupil responses. Is the child giving a picture of himself as he is, or as he would like to be, or as he would like to have others think he is? Is the pupil joking or is he in earnest? Is he intentionally trying to obscure certain facts through giving others? The very listing of items to be checked may be highly suggestive and prompt a response which

TABLE 1

	Public S 9th	Public S 12th	Private S 9th	Private S 12th
Number of pupils	115	67	58	36
Boys	60	36	29	16
Girls	55	31	29	20
Median age	15-0	17-9	14-7	17-6

might otherwise never have occurred. Several studies have indicated the fluctuating character of pupil interests as observed by means of questionnaires. The day, the week, the season of the year, the immediate preceding experiences, the pupil's emotional tone at the time of response, his physical well-being, all influence response to a greater or less degree. The influence of suggestion from chum or teacher, the subtle suggestion of the immediate environment may influence the child's responses in different ways on varying occasions. The way in which the material of the questionnaire is organized and presented may again influence the pupil's choice in such a way as to make the resulting picture unreliable Certainly a "paper-and-pencil" device suffers in comparison with long-time, carefully controlled observation of the pupil actually at work or play

Considerable fluctuation in pupil interests and activities during adolescence is typical and may be anticipated no matter how reliable the observation But fluctuations due to the technique of observation may tend to obscure the true picture of the situation. The validity and reliability of questionnaire responses can be determined in part through the inner consistency of results, and the degree to which the picture of the individual given by the questionnaire checks with other observations of the individual under similar conditions If many cases result in which pupils who are known to dislike school work and to prefer active sports to sedentary games give pictures of themselves on written questionnaires as pedantic scholars and inveterate bookworms the reliability of the questionnaire data is at once questioned If the number of cases in which wide discrepancy occurs is negligible and if the total findings give some approximation to results obtained by others, the results may be considered reliable enough for practical purposes. The reliability of the group picture may normally be expected to be higher than the reliability of the individual picture. Ordinarily the more mature the individual the more capable he is of giving a true statement of the facts uninfluenced by suggestion.

In the present questionnaire all items to be checked were arranged in alphabetical order. Only in the longest lists did there appear to be any tendency to concentrate responses more toward the beginning than toward the end of the list. Public-school pupils were somewhat more prone to neglect the last few items in the longer lists than were private-school pupils.

Results from the present questionnaire data indicate in the main

a serious attitude on the part of the pupils responding, and little attempt to give an imaginary picture in place of the real one. The questionnaire results tally well with known facts about individual pupils. Even the girl who asserted on the check list that she wished to become an "undertaker" insisted when questioned afterwards that she actually meant it In general, then, considerable significance may be attached to the results The questionnaires were given in the spring of the year, a fact which probably affected somewhat the choices under the heading of "sports"

Topics of Inquiry

The data furnish evidence concerning some of the following questions

1 The nature of individual differences in response for different school and grade groups.

2 Sex differences for the two grade levels in the two types of schools.

3 Comparison of upper- and lower-grade responses in the two types of schools.

4 Comparison of responses of pupils from different environments, at two different grade levels. This differs from item 3 in its emphasis on environmental differences in contrast to grade-level differences

5 Central tendency in frequency of response in the two types of schools at the two grade levels.

6. Comparison of responses for pupils of similar age but of relatively high and low mental ability.

Summary of Questionnaire Responses and Test Results

The experimental tests described in an earlier paragraph measure a combination of intelligence and achievement factors, but since the latter is largely dependent upon the former any differences found may be considered differences in mental maturity In interpreting results, the fact that at both grade levels private-school pupils were on the average several months younger than public-school pupils of the same grade level makes it necessary to give private-school pupils allowance for the age factor.

The median total test scores for all pupils are shown in Table 2.

TABLE 2

	Public S 9th	Private S. 9th	Public S. 12th	Private S. 12th
Number of cases	115	58	67	36
Highest	96	121	133	143
Q^3	59.6	99.2	99	130
Median	49.6	83.3	82.5	117.5
Q^1	43.6	73.5	64.0	100
Lowest	24	53	37	66
Q	8	12.9	17.5	15

These scores indicate the decided superiority of the private-school group over the public-school group in mental maturity. When the age factor is taken into account the actual differences in relative ability are still greater. Even the direct comparison of scores given above indicates that the ninth-grade private-school pupils rank slightly above the twelfth-grade public-school pupils. These results are in harmony with other reports of the mental superiority of private-school pupils in general This superiority is no doubt due to both environmental and native ability factors, but whether one set of factors plays a larger part than the other has not been conclusively determined Results show that even at earliest school entrance private-school pupils are a highly endowed group and favorable opportunity no doubt enables such pupils to make the most of their abilities On the other hand, opportunity to use ability has not been lacking in the public-school group for they have attended modern, well-equipped schools, have ample free library facilities, and in the case of the particular group reported here have all the facilities of a nearby large city at their disposal.

In the following summaries of questionnaire results items with the five highest frequencies of votes are listed in order of frequency. No distinction is made between the order of pupils' choices, first, second, or third. In case of a tie in frequency, items of duplicate frequency are given. In making comparisons between the different groups it must be noted that all private-school groups are smaller than public-school groups at corresponding grade levels, and this factor may affect both the range of choice and the reliability of data

Following each list of items a statement is made of the range of choice of possible choices.

RECREATION—SPORTS

Public School—9th Grade

Order	Boys Activity	Frequency	Order	Girls Activity	Frequency
1st	Baseball	40	1st	Basketball	38
2nd	Basketball	34	2nd	Tennis	31
3rd	Football	29	3rd	Golf	22
4th	Handball	9	4th	Baseball	19
5th	Hockey	8	5th	Handball	12

Range of choice for boys and girls together, 29 of 43 items.

Private School—9th Grade

Order	Boys	Frequency	Order	Girls	Frequency
1st	Basketball	18	1st	Tennis	20
2nd	Baseball	13	2nd	Basketball	11
3rd	Tennis	9	3rd	Baseball	10
4th	Football	7	4th	Bridge	10
5th	Soccer	5	5th	Croquet	4
	Pingpong	5			
	Track	5			

Range of choice 27 of 44 items

Public School—12th Grade

Order	Boys	Frequency	Order	Girls	Frequency
1st	Baseball	21	1st	Tennis	21
2nd	Basketball	19	2nd	Basketball	16
3rd	Football	15	3rd	Bridge	9
4th	Boxing	7	4th	Golf	8
5th	Bridge	5	5th	Baseball	5
	Handball	5		Pingpong	5

Range of choice for boys and girls together 24 of 43 items

Private School—12th Grade

Order	Boys	Frequency	Order	Girls	Frequency
1st	Tennis	10	1st	Tennis	12
2nd	Basketball	7	2nd	Basketball	9
3rd	Chess	6	3rd	Bridge	7
4th	Handball	3	4th	Soccer	5
5th	Wrestling	3	5th	Baseball	4

Range of choice, 22 of 44 items.

The popularity of baseball and basketball was to have been anticipated. Tennis receives higher rank for both boys and girls in the private-school groups than in public school. Chess makes the list only in the case of twelfth-grade private-school boys. Bridge appears first in the votes of ninth-grade private-school girls, and ranks in the choices of both boys and girls in public school and in girls of the private-school group. A striking fact is the greater similarity than dissimilarity among the sports choices of both boys and girls at both grade levels. Must the concept of "femininity versus masculinity" disappear?

RECREATION—ACTIVITIES

Public School—9th Grade

Boys Order	Boys Activity	Boys Frequency	Girls Order	Girls Activity	Girls Frequency
1st	Swimming	30	1st	Swimming	30
2nd	Ice skating	22	2nd	Ice skating	29
3rd	Watching athletic events	18	3rd	Social dancing	20
4th	Bicycle riding	15	4th	Horseback riding	19
5th	Fishing	14	5th	Attending movies	14
	Camping	14			

Range of choice: 67 of 80 items

Private School—9th Grade

Boys Order	Boys Activity	Boys Frequency	Girls Order	Girls Activity	Girls Frequency
1st	Swimming	13	1st	Social dancing	21
2nd	Reading	9	2nd	Horseback riding	16
3rd and	Driving an automobile	8	3rd	Swimming	14
4th	Horseback riding	8	4th	Attending plays	9
5th	Ice skating	7	5th	Ice skating	7
	Bicycle riding	7			
	Social dancing	7			

Range of choice. 54 of 84 items

Public School—12th Grade

Boys Order	Boys Activity	Boys Frequency	Girls Order	Girls Activity	Girls Frequency
1st	Swimming	20	1st	Swimming	14
2nd	Driving an automobile	13	2nd	Ice skating	13
			3rd	Social dancing	11
3rd	Ice skating	8	4th	Driving an automobile	8
4th	Bicycle riding	5	5th	Horseback riding	7
5th	Fishing	5			
	Camping	5			
	Reading	5			

Range of choice. 61 of 80 items

Private School—12th Grade

Boys Order	Boys Activity	Boys Frequency	Girls Order	Girls Activity	Girls Frequency
1st	Driving an automobile	8	1st	Social dancing	14
2nd	Social dancing	7	2nd	Driving an automobile	9
3rd	Attending plays	6	3rd	Horseback riding	9
4th	Reading	5	4th	Swimming	8
5th	Having dates	4	5th	Attending plays	6
	Swimming	4		Attending movies	5
	Talking to people	4			

Range of choice: 47 of 84 items.

The list of items to be checked contained every important form of activity that could possibly appeal to an adolescent boy or girl and it is interesting to see the concentration of preference for the predominately physical activities and the comparatively infrequent choice of sedentary activities or passive amusements. The astonishing thing here is the infrequent mention of "movie" attendance in comparison with the vote for swimming, skating, and dancing. The total vote for all activities shows a wide range of choice in spite of the concentration of votes indicated There is somewhat more differentiation of the choices of boys and girls in this category than in the preceding one. Whether the vote for "reading" by ninth-grade private-school boys and that of "social dancing" by ninth-grade private-school girls with no corresponding vote in the public-school groups of the same grade level indicates difference in mental maturity or of opportunity is difficult to determine Other studies have indicated the tendency of brighter children to mention reading as a preferred activity earlier than average or duller children. There is also some corroborating evidence for the advanced social maturity that the choice of social dancing would indicate.

PREFERRED ASSOCIATES

Public School—9th Grade

	Boys		Girls	
1st	Boys your own age	44	Girls your own age	42
2nd	Mother	30	Mother	38
3rd	Father	25	Father	24
4th	Girls your own age	22	Boys your own age	18
5th	Older boys	10	Older boys	14

Range of choice 9 of 11

Private School—9th Grade

	Boys		Girls	
1st	Boys your own age	23	Girls your own age	22
2nd	Mother	12	Older boys	18
3rd	Father	12	Father	10
4th	Girls your own age	9	Mother	10
5th	Older boys	8	Older girls	9

Range of choice. 10 of 11.

Public School—12th Grade

	Boys		Girls	
1st	Boys your own age	26	Girls your own age	23
2nd	*Girls your own age*	*17*	Mother	18
3rd	Mother	15	Father	12
4th	Father	15	Boys your own age	11
5th	Older boys	9	Older boys	8
			Older girls	8

Range of choice 10 of 11

Private School—12th Grade

	Boys		Girls	
1st	Boys your own age	11	Older boys	16
2nd	Girls your own age	4	Girls your own age	9
3rd	Father	4	Older girls	4
4th	Mother	4	Other adults	3
5th	Younger girls	3	Boys your own age	3
	Other adults	3		
	Range of choice 9 of 11			

The increasing tendency of girls toward heterosexual choices as they progress from ninth to twelfth grade is indicated here Private-school girls tend to show this trend earlier than others. "Father" and "mother" disappear from the list in the choices of the twelfth-grade private-school girls Since the number of pupils here is small, this fact may be characteristic only of the group reported, and may not be typical of a larger number of twelfth-grade girls selected at random.

SCHOOL SUBJECTS

Public School—9th Grade

	Boys		Girls	
1st	Civics	25	Typewriting	27
2nd	General science	24	English	22
3rd	English	18	General science	20
4th	Algebra	15	Civics	19
5th	Physical education	10	Physical education	11
	Shop work	10		
	Range of choice: 34 of 57 items			

Private School—9th Grade

	Boys		Girls	
1st	Algebra	20	English	17
2nd	General science	10	French	16
3rd	Social studies	8	Social studies	10
4th	French	7	Fine arts	8
5th	Physical education	7	Physical education	5
			Household arts	5
	Range of choice: 26 of 43 items			

Public School—12th Grade

	Boys		Girls	
1st	Algebra	11	English	10
2nd	American History	11	Typewriting	10
3rd	Biology	10	Physical education	8
4th	Physical education	9	Biology	7
5th	English	8	Shorthand	7
	Range of choice 35 of 57 items.			

Private School—12th Grade

	Boys		Girls	
1st	English	7	English	12
2nd	Algebra	6	French	10
3rd	Biology	4	Fine arts	8
4th	Physics	4	Algebra	6
5th	Trigonometry	4	Physical education	5

Range of choice 25 of 43 items

Any comparison between the two types of schools at both grade levels is difficult to make because of differences in curricula. In the private school, typewriting is not offered and social studies takes the place of civics In other respects, except for grade placement of subjects the courses of study of the two schools are not radically different Fine arts, physical education, and general science are offered in both Pupils were asked to check only the subjects they had actually taken and the range of possible choice is necessarily limited by this factor The child who had never studied algebra was not in a position to vote whether he liked it or not Subject requirements differ somewhat in the two schools Nevertheless, there is sufficient range of choice and overlapping between the two schools to admit of comparison in general trends The chief apparent difference between the two types of schools is the preference of the ninth- and twelfth-grade public-school girls for commercial subjects, in contrast to the more academic choices of private-school girls. As will be seen later, these preferences correspond with vocational preferences and higher education choices.

Choice of School Following High School

Public School—9th Grade

	Boys		Girls	
1st	State university	10	Business school	11
2nd	State engineering school	8	Normal school	9
3rd	Large men's college	6	No school	5
4th	No school	6	Small women's college	3
5th	Business college	4	Dramatic school	3
	Medical school	4		

Range of choice 17 of 28 items

Private School—9th Grade

	Boys		Girls	
1st	Large men's college or university	18	Large women's college or university	9
2nd	State engineering school	7	Large coeducational college	7
3rd	Medical school	4	Small coeducational college	6
4th	Small coeducational college	2	Art school	3
5th	State agricultural school	1	Small women's college	3
	Large coeducational college	1		
	Small men's college	1		

Range of choice. 15 of 26 items

Public School—12th Grade

	Boys		Girls	
1st	Large men's college	6	Large women's college	6
2nd	Large coeducational college	6	No school	5
3rd	Normal school	3	Teachers college	3
4th	State university	3	Business college	3
5th	Small men's college	3	Music school	3
			Law school	3

Range of choice 21 of 28 items

Private School—12th Grade

	Boys		Girls	
1st	Large men's college or university	10	Small women's college	7
2nd	Small men's college	2	Large coeducational college	4
3rd	State university	1	Large women's college or university	3
4th	Small coeducational college	1	Normal school	2
5th	Music school	1	State university	1
	Dramatic school	1	Small coeducational college	1
			Music school	1
			Finishing school	1
			Dramatic school	1
			No school	1

Range of choices. 9 of 26 items

The results indicate that the public-school girls of both grade levels tend more strongly than private-school girls to prefer commercial and professional training rather than academic college work following high school. This fact is in harmony with school-subject

preferences summarized above. "No school" following high school is the choice of 16 public-school pupils and of 1 private-school pupil, considering both grade levels together. In other respects the two types of schools show similar choices.

Types of Reading Preferred

In the questionnaire, the pupils were asked to list several books they had read during the past year or two. They were then asked to check in a list of 30 types of reading material the kind they preferred. Because of its length the list of books read cannot be included here, but a summary will be given of the types of reading material preferred.

Public School—9th Grade

	Boys		Girls	
1st	Mystery stories	25	Mystery stories	31
2nd	Adventure stories	18	Novels and love stories	24
3rd	Short stories	14	Short stories	15
4th	Science	6	Adventure stories	10
5th	Travels	4	Directions for making things	5
	Novels and love stories	4		
	Encyclopedias	4		

Range of choice 20 of 30 items

Private School—9th Grade

	Boys		Girls	
1st	Mystery stories	9	Novels and love stories	15
2nd	Adventure stories	8	Mystery stories	10
3rd	Science	4	Historical novels	8
4th	Historical novels	3	Adventure stories	5
5th	Biography	3	Short stories	5

Range of choice 15 of 22 items

Public School—12th Grade

	Boys		Girls	
1st	Mystery stories	18	Mystery stories	15
2nd	Adventure stories	13	Short stories	6
3rd	Novels and love stories	6	Novels and love stories	6
4th	*Short stories*	6	Plays	6
5th	Historical novels	5	Adventure stories	4
			Philosophy	4
			Biography	4

Range of choice, 18 of 30 items

Private School—12th Grade

	Boys		Girls	
1st	Mystery stories	3	Novels and love stories	12
2nd	Short stories	3	Biography	5
3rd	Philosophy	2	Plays	4
4th	Poetry	2	Historical novels	3
5th	Novels and love stories	1	Adventure stories	2
	Folk tales and legends	1	Travel stories	2
	Science	1		
	Humor	1		

Range of choice, 15 of 22 items

The popularity of adventure and mystery stories with both boys and girls at both grade levels and in both types of schools is at once apparent Novels and love stories receive a higher rating from girls than from boys in both schools and at both grade levels.

Magazine Preferences

Public School—9th Grade

	Boys		Girls	
1st	*Popular Science*	22	*Ladies Home Journal*	12
2nd	*Boy's Life*	18	*Literary Digest*	11
3rd	*American Boy*	18	*Good Housekeeping*	8
4th	*Popular Mechanics*	18	*Motion Picture Classics*	8
5th	*Detective Stories*	10	*College Humor*	7
	Literary Digest	10	*Cosmopolitan*	7

Range of choice 61 of 126 items

Private School—9th Grade

	Boys		Girls	
1st	*American Boy*	10	*New Yorker*	17
2nd	*Popular Science*	9	*Saturday Evening Post*	9
3rd	*Boy's Life*	7	*Judge*	6
4th	*National Geographic Magazine*	5	*Life*	6
5th	*Popular Mechanics*	5	*College Humor*	3
	Saturday Evening Post	5	*Cosmopolitan*	3
			Good Housekeeping	3
			Harper's Bazaar	3
			Vanity Fair	3

Range of choice, 51 of 123 items

Public School—12th Grade

	Boys		Girls	
1st	*Literary Digest*	15	*Literary Digest*	11
2nd	*Collier's*	9	*American Girl*	7
3rd	*Boy's Life*	7	*Collier's*	6
4th	*College Humor*	7	*Cosmopolitan*	5
5th	*American Boy*	6	*Good Housekeeping*	5
	Popular Mechanics	6		

Range of choice. 52 of 123 items.

Private School—12th Grade

	Boys		Girls	
1st	*Judge*	4	*New Yorker*	12
2nd	*New Yorker*	4	*Saturday Evening Post*	7
3rd	*Saturday Evening Post*	4	*Life*	6
4th	*Life*	3	*Literary Digest*	5
5th	*American Mercury*	2	*Theatre Magazine*	4
	Collier's	2		
	Popular Science	2		
	Scribner's	2		
	Time	2		
	Science News Letter	2		
	Harper's	2		

Range of choice 39 of 123 items

Private-school boys of both grade levels show more serious reading tastes in their magazine preferences than either private-school girls or public-school boys and girls. Ninth-grade private-school girls show somewhat more sophistication in their choices than private-school boys or public-school boys and girls Cheap and sensational publications are mentioned more frequently by both boys and girls in public-school ninth-grade than the corresponding grade in private school. A summary of all choices shows that the *Literary Digest* is the most widely read, *Popular Science* is second, *The American Boy,* third, and the *New Yorker,* fourth in popularity In evaluating choices and in making comparisons the question of the availability of reading matter arises The school and public libraries available to pupils of both groups are comparable, but the Sims Score Card data for private-school pupils indicates the larger quantity and superior quality of reading matter in their homes as compared to that of public-school pupils

VOCATIONAL PREFERENCES

Public School—9th Grade

	Boys		Girls	
1st	Aviator	12	Stenographer and typist	16
2nd	Baseball player	12	Nurse	13
3rd	Chemist	8	Private secretary	12
4th	Civil engineer	8	School teacher	10
5th	Forest ranger	6	Bookkeeper	6
	Lawyer or judge	6	Dancer	6

Range of choice: 79 of 130 items

Private School—9th Grade

	Boys		Girls	
1st	Lawyer or judge	10	Actress	7
2nd	Business executive	6	Costume designer	6
3rd	Doctor	5	Dancer	6
4th	Mechanical engineer	5	Interior decorator	6
5th	Scientific research worker	5	Novelist	5

Range of choice 61 of 136 items

Public School—12th Grade

	Boys		Girls	
1st	Baseball player	6	Stenographer or typist	11
2nd	Aviator	5	School teacher	8
3rd	Lawyer or judge	5	Nurse	8
4th	Journalist	4	Private secretary	8
5th	School teacher	4	Athletic director	5
	Forest ranger	4		

Range of choice: 67 of 130 items

Private School—12th Grade

	Boys		Girls	
1st	Banker	3	Actress	5
2nd	Actor	2	Social worker	5
3rd	Chemist	2	Musician	4
4th	College professor	2	Artist	3
5th	Lawyer	2	Homemaker	3
	Novelist	2		
	Playwright	2		
	Psychologist	2		

Range of choice 32 of 126 items

Private-school boys of both grade levels show greater preference for the highly trained professions than do public-school boys. Commercial preferences again lead in the public-school girls' groups Artistic preferences are more apparent in the choices of private-school

girls of both grade levels A wide range of choice was expressed by all pupils in both types of schools There is a high degree of consistency between the vocational preferences of public-school pupils and their reading, school subjects, and higher education preferences Consistency is less apparent in the same categories for private-school pupils.

Comparison of Results for Groups of More and Less Intelligent Pupils

Although there is considerable overlapping in the mental ability represented in the public- and private-school groups, the series of mental alertness tests given to all pupils indicated the general mental superiority of the private-school group In order to make a more specific comparison of the choices of brighter and duller pupils, two groups of pupils were selected from the entire range of data Group I consisted of pupils who were between 15 and 16 years of age and who had the 20 highest total scores on the mental alertness tests Group II consisted of the 20 lowest scores on the mental alertness tests who at the same time were between 15 and 16 years of age. Pupils were selected without reference to whether they came from the private- or public-school situation, but after the material was collected it was found that none of the duller groups were in the private-school list and none of the brighter were among the public-school pupils In order to have a sufficient number of cases of brighter pupils to match with the less capable pupils, a few who met the criteria were selected from eleventh-grade private-school data A larger number of cases would have insured greater reliability in any differences found between the two groups, but an increase in the number of cases would have meant considerable decrease in the difference in mental ability of the two groups. By choosing the extremes of the distribution of similar age range there is practical certainty that there is a real difference in the mental ability of the two groups Since equal percentages of girls and boys are found in the two groups the results will be summarized without reference to sex differences There were 7 girls and 13 boys in each group. The median total weighted score of the bright group was 113 and of the dull group, 39. Reference to the test data given earlier for all groups will indicate the relation of these scores to the total range of scores and to the probable error which is expressed approximately

in terms of "Q" The summary of questionnaire choices for the 20 pupils of both groups in all categories, using first choices only, is as follows.

GAMES

Group I		Group II	
Tennis	5	Baseball	10
Baseball	3	Basketball	7
Basketball	3	Hockey	2
Pingpong	2	Billiards	1
Chess	1		
Track	1		
Backgammon	1		
Waterpolo	1		
Soccer	1		
Golf	1		
Hockey	1		

Group I shows a wider range of choice than Group II and the choices of greatest frequency are not in the same order as those of Group II

OTHER RECREATIONS

Group I		Group II	
Traveling	4	Swimming	3
Swimming	3	Ice skating	3
Doing science experiments	2	Going to church or Sunday School	2
Having dates	2	Fishing	2
Reading	1	Folk dancing	1
Hiking	1	Social dancing	1
Attending plays	1	Watching athletic events	1
Skiing	1	Horseback riding	1
Working around machinery	1	Traveling	1
Boating	1	Attending movies	1
Horseback riding	1	Gymnasium apparatus work	1
Going to parties	1	Drawing or painting	1
Camping	1	Racing	1
		Playing radio or Victrola	1

Considerable range of choice is noticeable in both groups and there is not much overlapping in activity preferences

CHOICE OF ASSOCIATES

Group I		Group II	
Person of the same age or sex	10	Person of same age or sex	14
Girls preferring older boys	4	Mother	4
Boys preferring older boys	4	Girls preferring older boys	1
Girls preferring younger boys	1	No response	1
Mother	1		

Choice of School Subjects

Group I		Group II	
Science	6	Typing or commercial	4
English	5	Civics	4
Mathematics	3	English	3
History	3	Science	3
French	2	Fine arts	3
Dramatics	1	Shop	1
		History	1
		Household arts	1

School Preference after High School

Group I		Group II	
Large men's college	6	Music or art school	4
Small coeducational college	4	Business college	3
Large women's college	3	State university	3
Large coeducational college or university	2	No vote	3
Small women's college	2	Trade school	2
State engineering school	2	Large men's college	1
State university	2	Junior college	1
Choice not yet made	1	Teachers college	1
(Two pupils voted for two each)		Not going	1
		State agricultural school	1

Vocation Preferred

Group I		Group II	
Scientific research worker	3	Accountant	2
Engineer	3	Stenographer	2
Author	3	Cartoonist	2
No vote	2	Dancer	1
Banker	2	Costume designer	1
Lawyer or judge	1	Journalist	1
Social worker	1	Novelist	1
Architect	1	Banker	1
Egyptologist	1	Baker	1
Etomologist	1	Reporter	1
College professor	1	Business executive	1
Actor	1	Draftsman	1
		Explorer	1
		Football coach	1
		Nurse	1
		Baseball player	1

Kind of Reading Preferred

Group I		Group II	
Novels and love stories	8	Mystery stories	6
Mystery stories	2	Adventure stories	5
Short stories	2	Novels and love stories	2
No choice	2	Classics	2
History	1	Philosophy	1
Historical novels	1	Directions for making things	1
Nature and animals	1		
Folk tales and legends	1		
Biography	1		
Science	1		

MAGAZINES PREFERRED

Group I		Group II	
New Yorker	4	*American Boy*	5
National Geographic	3	*True Stories*	3
Saturday Evening Post	2	*Cosmopolitan*	2
Readers' Digest	2	*Detective Stories*	2
Asia	1	*No vote*	2
Literary Digest	1	*Popular Science*	1
Golden Book	1	*College Humor*	1
Sport Story Magazine	1	*Field and Stream*	1
Theatre Guild Magazine	1	*Open Road*	1
Harper's Bazaar	1	*American Girl*	1
Popular Science	1	*Saturday Evening Post*	1
Life	1		
American	1		

A check of the books listed as read during the past year or two indicates a larger amount of required school reading listed by the duller group and a larger amount of adult novel reading on the part of the brighter group. There was some overlapping on both groups. There was more mention of the same books by the duller group than by the brighter group. The brighter group showed a wider range in reading taste

In the foregoing summaries of preferences there is some overlapping in each category of choice but also considerable difference. Some of this difference appears to be in harmony with the differences in mental ability the two groups show On the other hand, some of the difference may be due to the chance factor in the small number of cases Some of the difference found is not necessarily related to mental maturity, though unquestionably to that factor the largest share of the difference must be atttributed. Whether these differences are native or are the product of opportunity and environmental factors cannot be determined from these data. In the main the results for the two selected groups are similar to results for the larger public- and private-school groups whose choices were first summarized.

SUMMARY AND CONCLUSION

1. By means of a questionnaire and a series of experimental mental alertness tests data were obtained from pupils of ninth- and twelfth-grade levels in both a private-school and a public-school system. Responses were recorded for several types of activity and interest preferences. sports, recreational activities, school-subject pref-

erences, reading and magazine preferences, choice of associates, higher education and vocational preferences. Public-school pupils of both grade levels, were, on the average, several months older than private-school pupils. Experimental test results indicated the superiority of the private-school group over the public-school group in mental maturity The ninth-grade private-school group exceeded the median score of the twelfth-grade public-school group by a small amount.

2. The chief differences in questionnaire responses between public- and private-school groups were in the school-subject, vocational choice, and reading preference categories In all categories there was considerable overlapping in the choices of pupils of the two types of schools The private-school ninth-grade girls showed the highest degree of consistency in all categories They revealed in their choices more maturity and sophistication than the public-school girls of the same grade level Whether the differences found are to be attributed to native ability or to opportunity or to both factors in definite proportions cannot be determined from the results. The results found may be considered reliable enough to give suggestions to educators who are planning academic curricula or improving recreational facilities for adolescent pupils The problem of developing interests in pupils of adolescent age is dependent upon factors of initial interest and preference, and to the whole problem of mental maturity and environmental conditions

3. Two groups of pupils chosen so as to be comparable in age but at the extremes of the total distribution in mental maturity show some overlapping in preferences, but characteristic differences as well. These differences may be attributed in part to mental maturity factors, but may also be due in part to chance selection and to environmental circumstances

4. The present study indicates the need for the investigation of some of the factors on a more elaborate scale and the need of further research in the use of the questionnaire as a research instrument. Suggested topics of investigation are. fluctuations in interests over periods of time of varying length, seasonal variations in sports, influence of the length of the questionnaire on pupil responses, the relationship between questionnaire choices and actual behavior, sex differences of pupils in coeducational institutions as compared with pupils in separate institutions, the relation of personality factors to questionnaire responses, the relationship between reading interests and ability levels, responses of children of similar backgrounds but of

different ability, college and vocational choices and subsequent selection.

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Teachers College
Columbia University
New York City

LES INTÉRÊTS ET LES HABILETÉS DES ADOLESCENTS

(Résumé)

On a obtenu des renseignements à l'égard des intérêts et des activités des élèves de l'âge adolescent dans des "high schools" au moyen d'un questionnaire rempli par les élèves assistant à deux types d'école—une "high school" publique, dans l'un cas, et une école privée progressive dans l'autre Dans le dernier cas les élèves ont été de familles d'un état social et économique meilleur que le moyen On a déterminé la capacité mentale et le rendement scolaire de tous les élèves au moyen de tests objectifs Les résultats ont indiqué la supériorité des groupes de écoles privées sur tous les tests On a demandé aux élèves de donner des renseignements à l'égard de leur intérêt pour les jeux physiques et les autres types d'activités, leurs compagnons préférés, les matières scolaires, leurs préférences scolaires après avoir assisté à la "high school," leurs lectures préférées, et leurs préférences

professionnelles Les résultats montrent que dans le cas des préférences pour les jeux physiques il existe comparativement peu de différence dans les choix des garçons et des filles D'entre les autres activités celles qui sont principalement d'un caractère physique sont preférées aux récréations sédentaires ou passives Les éleves des écoles privées préférent la lecture plus que les élèves des écoles publiques. Les différences des matière scolaires préferées reflètent les différences des cours d'études des deux types d'ecole Les élèves de l'école publique montrent moins de préférence pour les matières strictement scolaires que les elèves de l'école privée. On peut attribuer les différences trouvées aux facteurs de maturité mentale, de conditions de milieu, et circonstances fortuites

HILDRETH

DIE INTERESSEN UND FAHIGKEITEN JUGENDLICHER

(Referat)

Es wurden Daten gesammelt in Bezug auf die Interessen und Tatigkeiten jugendlicher Schuler aus hoheren Schulen durch Verwendung eines Fragenbogens der von Schulern aus hoheren Schulen von zwei Sorten—einer offentlichen hoheren Schule und einer "avancierten" [progressive] Privatschule—beantwortet wurde Die Schuler der letzteren stammten aus *Heimen, deren sozialer und ekonomischer Stand besser war, als der durchschnittliche* Die geistige Leistungsfahigkeit und die Schulleistung aller Schuler wurden an objektiven Tests festgestellt Die Befunde wiesen auf die Überlegenheit der Privatschulgruppen in allen Tests hin. Von allen Schulern wurde Bescheid erhalten uber ihre Interessen in Bezug auf Sport und ander Formen der Tätigkeit, und uber bevorzugte Schulgefahrten, Studien, weitere Fortbildung, Lekture, und Beruf Die Befunde erwiesen, dass in Bezug auf Sportbevorzugungen relativ wenige Unterschiede zwischen Knaben und Madchen bestehen. Unter anderen Tatigkeiten sind diejenige, die vorwiegend korperlicher Art sind, beliebter, als sesshafte oder passive Unterhaltungsweisen Die Privatschuler erweisen eine starkere Neigung zur Lekture, als die Schuler offentlicher Schulen Die Unterschiede in Bezug auf die bevorzugten Studien gehen mit Unterschieden zwischen den Kurrikulen der beiden Schularten einher Die Schuler der offentlichen Schule zeigen weniger Vorliebe fur die streng akademischen Schulstudien als die Privatschuler Die erwiesenen Unterschiede sind vielleicht auf die Einwirkungen der geistigen Reife, der Umgebung, und zufalliger Umstande zuruckzufuhren

HILDRETH

MODERN TRENDS IN THE PSYCHOLOGY OF MALADJUSTED SCHOOL CHILDREN*

From New York City

MARION JENKINS

The rapid progress in recent years as regards our understanding of the maladjusted child has been coordinate with and, in part, dependent upon, rapid advances in the general field of child behavior. These advances have been characterized not only by large accretions to our stock of fact and generalization, but also by a record of achievement in devising methods for the study of child behavior and for the diagnostic-therapeutic approach to the problems of maladjustment. Hence, a knowledge of not only the results of research, but also of the methodological and diagnostic approaches, is necessary for a grasp of the modern trend in psychology of behavioral maladjustments. Consequently, in the present paper an attempt is made to analyze the present status and trend of the psychology of problem behavior from three points of view. (1) methods of acquiring data, (2) the causal and genetic factors involved in child maladjustments, and (3) the diagnosis and treatment of behavioral disorders

1. METHODS FOR INVESTIGATING MALADJUSTED BEHAVIOR

The methods for attacking the problems of child maladjustment are, in large part, the methods of studying child behavior in general, a fact which is due, in part, to the circumstance that general methods of investigating child behavior can be applied directly to the problems of maladjustment, and, in part, to the importance of child psychology for a thorough understanding of problem behavior. At the present time, the investigation of child behavior is characterized not only by a gradual increase in the use of experimental situations, but also by a very rapid development of special non-experimental methods and techniques for recording data that are both comparable and reliable.

The subject-matter of psychology, unlike that of the physical sciences, is such that it is very difficult or impossible to be aware of

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all the factors which are essential for the behavioral adustments of an organism. Failure to give sufficient attention to this fact has resulted in a large body of fruitless investigations in the field of child behavior, a fact which at present is stimulating an increased emphasis upon the use of methods for neutralizing the influence of uncontrolled and unknown factors. These general scientific methods are applicable to both the non-experimental and experimental approaches to the study of child behavior. The most important of these is the method of *equivalent groups* which involves the equating of two or more groups either by chance (random sampling) or by equating for various factors, such as intelligence, age, sex, etc Examples of equivalent-group procedures are those involving the use of a control group and the so-called cross-sectional methods of determining norms of child development The latter method consists of a study of different groups of subjects at varying age levels instead of the observation of the same group at different stages of development. Secondly, there is the method of *rotation*, whereby data recorded from a single group are made more reliable by rotating out the effects of certain factors, as, for example, practice and fatigue.

The following synopsis includes the methods (except the general ones just mentioned) that are in general use at the present time for the investigation of the behavioral adjustments of children It will be noted that some of the methods classed as non-experimental are sometimes referred to by certain writers as experimental, because of the use of controlled observation. The writer believes, however, that it is more satisfactory and less confusing to reserve the term experimental for those methods which not only may involve "control of the observer" but also control of the stimulus situation

I Non-experimental methods. Well exemplified by the natural-history approach to child problems Do not involve control of the situation surrounding the subject, but may involve various types of controlled observation

- *A* Historical procedures
  - 1 The interview
    - *a* Crude interview
    - *b* Psychoanalytic interview. Attempts to elicit a retrospective account regarding experiences of childhood
    - *c*. Systematic interview These may be either (1) itemized, as, for example, the "analytic interview" of Tjaden (55), or (2) fixed question interviews in which the questions of the interviewer are either spoken or presented in written form

2 Biographical May consist either of comparisons of published biographies and letters, or systematic biographical sketches of behavior, such as Preyer's record of the first three years of the life of his son

a Adult reminiscences regarding childhood experiences, as, for instance, Hall's monograph regarding early memories

b. Autobiographical essay

1) General Well exemplified by the procedure of Selling (47), who asks the incoming clinic child to write a story of his life, making drawings when necessary

2) Specific. Limited to some phase of the child's experience, a procedure which was utilized by Levy and Munroe in their investigation of the day-dreams of Chicago school children.

4. Questionnaire, when used to obtain descriptions of the past be-behavior of children

a Personal Deals with the past history of the person answering

b Third party Example parental questionnaire regarding the parents' children.

5 Case history. A derived procedure involving the use of interviews, questionnaires, etc, and therefore merely an intensive study of an individual, especially adapted to the needs of clinics. Consists of data gathered through clinical interviews with the patient and contacts of social workers, teachers, etc, with the patient's parents, friends, etc Should be distinguished from the *case study*, which not only involves historical material, but also includes other pertinent data, such as those derived by means of psychometric techniques

B Observational procedures Differ from the historical in that the results, instead of being drawn from historical records, are based upon contemporaneous behavior records.

1. The interview Crude and systematic

2 The diary. A daily record of child behavior. May be either casual or continuous

a. Personal A diary recorded by children. Rare.

b Investigator and third-party diaries

1) Crude

2) Itemized

3. The observation chamber used by Gesell (14) and others. A child or group of children is placed in a room-like chamber and observed through a one-way vision screen Eliminates the variable influence of the presence of the observer

4 The questionnaire

5 Rating scales
  a Ranking (order of merit).
  b Categorized (1) Point or numerical, (2) graphic, (3) antithetic alternative, sometimes in yes-or-no form as in the case of the Woodworth-Mathews psychoneurotic inventory.
  c. Situational or concrete The individual is rated on what he would do in a given concrete situation
  Example the Allport A-S test
6 Systematic or controlled observation. According to this procedure, the observer attempts to secure more accurate data by recording according to a predetermined plan. According to D S Thomas (53), the central methodological problem here is "control of the observer" Should be distinguished from experimental methods which not only include control of the observer, but also control of the stimulus situation
  a Individual response techniques, as, for instance, the procedure of Loomis (53) for recording the physical contacts of nursery-school children
  b. Social situation techniques (situational analysis)
  Example the record of laughter situations by Gregg, Miller, and Linton (53).
  c Sampling techniques Example. Olson's (38) method of recording nervous activities
  d Simultaneous observation Utilized by Gesell for determining differences between behavior levels For instance two *children of different ages are observed at the same time* to note differences in behavior
7 Simple measurements Well exemplified by the anthropometric measurements of physical traits.
8. Comparison of the results of therapeutic treatment
9 Community surveys Example Shaw's (49) study of delinquent areas in Chicago

II Experimental methods Involve not only a modicum of control over the observer, but also control of the stimulus situation
  A Methods for testing receptor capacities
    1 Indirect techniques Useful for very young children
      a Conditioned response
      b Sorting techniques, such as those used by Baldwin in the investigation of color discrimination in the case of nursery children
    2 Direct techniques Similar to those commonly used in the case of adults To be used on children who make an intelligent use of language and who will follow directions

- *B.* Methods for testing reactive capacities
  1. Psychometric techniques Standardized for procedure and results. Useful for grading behavior and for determining individual differences
     - *a.* Tests of intellectual traits
       1) General ability
       2) Special aptitudes
     - *b.* Achievement tests. Educational, diagnostic, prognostic, etc
     - *c* Tests of non-intellectual traits Include measures of emotional instability, personality, character traits, suggestibility, motivation, etc
  2. Measures of modifiability, motivation, etc For very young children, such methods as the conditioned reaction and card sorting are the most satisfactory. For older children, the procedures are similar to those for adults

The limits of this paper preclude an extensive discussion of recent advances in methodological procedure, but it will be of interest to indicate briefly not only the trend of modern methodological developments but also the nature of the attempts to produce valid and reliable behavior records. In the first place, there has been a revival of interest in investigations involving non-experimental methods, a reaction which is due in part to a growing conviction that some extremely important phases of behavior are difficult, if not impossible, to study by means of experimental procedures The strength and the popularity of the experimental approach have motivated the present generation of child psychologists to determine the reliability and validity of behavior data secured in connection with the natural-history approach.

As a result of this critical attitude, many of the crude procedures of the past, such as the unsystematic interview, the biography, the abstract questionnaire, etc., claim very little attention except in so far as they suggest problems which may be attacked by more systematic methods. It is probably because of this critical attitude, in part, that the large literature with reference to psychoanalysis is generally considered as being significant only to the extent that it offers a mine of hypothesis-forming materials. Thus, Anna Freud (12) offers the variable procedure of establishing rapport as a part of the psychoanalytic *method* of examining children. The establishment of rapport has long been known in psychological circles primarily as an *art* of the professional psychometrist, and, since it must

be varied to suit the needs of the individual in the given situation, there is little warrant for calling such a variable procedure a scientific method

The modern attempt to secure comparable data by recording in accordance with a predetermined plan is no guarantee of obtaining valid and reliable records of behavior. The most careful investigators in the field of child behavior are now examining their proposed methods by means of sampling and statistical analysis. In the first place, the use of statistical analysis enables one to make use of methods of recording data which will give consistent and comparable results in the hands of different observers and in the case of repeated observations by the same observer. Secondly, it has led to a great saving of time as a result of special sampling techniques, such as that proposed by Olson (38) and discussed at length by Goodenough (16)

II Types, Causes, and Genesis of Maladjustments

Maladjustment and Heredity. Sustained interest in problem behavior was first directed to those outstanding cases of social maladjustment comprising offenses punishable by law The legal classification of certain individuals as criminals led several criminologists to the belief that criminality is determined by an hereditary background. The famous criminologist, Lombroso, as the result of extensive anthropometric measurements, formulated the theory of criminal types. According to this view, the diagnosis of criminality can be made on the basis of physical stigmata The theory of criminal types continued to be a subject of heated controversy until the publication of Goring's (18) investigation of convicts in English prisons Through the use of refined statistical methods and a control group of 1000 university students, he was able to show that there were no significant physical differences between the academic group and the English convict

Although the work of Goring indicates that there is no marked hereditary basis for criminality regarding physical traits, nevertheless this important study does not settle the question as to whether certain individuals are born criminals Futile attempts to reform criminals fostered the view that criminality may be due to moral imbecility The fullest expression of this theory is that of Tredgold (56) who argues at length that individuals of normal or superior intelligence may exhibit a lack of capacity for moral discrimination.

Although popular for a time, this theory has not been verified by clinical and psychometric evidence. The intensive investigation of delinquency on the part of Healy (20) failed to furnish any clear-cut instances of moral imbecility Moreover, attempts to use the ethical discrimination test of Kohs and others for diagnostic purposes have not only yielded negative results but also indicate that ability to make ethical judgments is closely related to intelligence

The close relationship between moral judgment and intelligence would seem to indicate that the latter might be the more important factor in criminal behavior. Goring, without using test methods, suggested that criminality was not due to "inherent wickedness, but natural stupidity." Soon after the publication of the famous work of Tredgold, Goddard and Terman (52), making use of revisions of the Binet test for intelligence, found that not only were delinquents below average in intelligence but also that a large percentage of these criminal groups were feebleminded. These findings led criminologists, lawyers, judges, and social workers to place undue emphasis upon mental deficiency as a factor in delinquency Critics soon pointed out that, granting the mental deficiency of delinquents, this in itself is not a certain argument for a causal relationship between mental defectiveness and criminality since the more intelligent individual would be more likely to escape apprehension and conviction. As the result of the recent and more careful measurements of the intelligence of juvenile and adult offenders, the theory that feeblemindedness is the most important causal factor in delinquency has lost ground. Pintner (40), in a series of summary tables of the work performed since 1911, showed that for the first seven years (1911-1917) the median percentage of delinquents as feebleminded is 45, whereas for the next twelve years the median percentage is only 21 This long-drawn-out controversy could have been settled long ago if some of these enthusiasts had taken the trouble to compare their results with those from a non-delinquent control group. Apparently the early American revisions of the Binet scale were inadequately standardized for the higher age levels. For instance, when the Stanford Revision was given to an unselected army group, the average mental age was approximately 13, a fact which indicated that early investigators were setting too high a standard for their delinquent groups Recent group tests with the Army Alpha have shown again that the differences, if any, between delinquents and non-delinquents are relatively small. Burt (5), the only investigator who made use of a large

control group of non-delinquent children, found that among delinquents 7.6% were feebleminded, whereas among non-delinquents 1.2% were feebleminded. These results indicate that feeblemindedness may be an important factor, but that it is not the only and probably not the major cause of delinquency.

Intelligence level is less a factor in producing crime than a determinant of the type of crime that will be committed. Other things being equal, the lower the intelligence, the sillier the crime. As Burt says, "to be successful, even in wrongdoing, needs a certain minimum of sense."

The hereditarian point of view is also adopted by those writers who argue that psychopathic constitutions (39) and innate emotional instability (5) are the primary causal factors in delinquency. Healy and Bronner (21), as well as Burt, have found a relatively small proportion of delinquent psychopaths. On the other hand, both Burt (5) and Slawson (51) have found a fairly high relationship between emotional instability and delinquency. The work of Slawson and others, however, indicates that the genesis of such traits as emotional instability may be determined in large part by early environmental factors. As a matter of fact, the bulk of the evidence at the present time indicates that problem behavior evolves as a natural process through the interaction of the organism and its environment. Experiments on babies have shown that extensive emotional conditioning occurs very early in life, and other studies of both preschool and school children indicate that social maladjustments are learned. Such facts as these have led inevitably to the conclusion that the understanding and the solution of behavior problems must be based upon a thorough examination of early child behavior, a fact which points to the diagnostic and therapeutic significance of investigations of preschool behavior.

Behavior Tendencies of the Preschool Child. The pioneer investigations of Watson and Rayner (60) concerning the emotional reactions of the human infant furnish evidence of extensive emotional conditioning very early in life. These early experiments gave psychologists a new insight into the importance of learning in the development of problem behavior. Many of the problem tendencies of school children probably have their roots in the developmental stages of the human infant. As a matter of fact, it is quite possible that emotional conditioning may persist even after the child has forgotten the environmental situation in which the conditioning first

occurred The artificial situation set up by the experimentalist for the study of preschool children provides a means of achieving a more thorough understanding of the problems of the developing child

The psychoanalysts, in attempting to unearth childhood memories, have directed attention to the possible significance of childhood experiences in the behavioral maladjustments of adolescents and adults. The psychoanalysts, in their extensive writings, have suggested many problems in the field of motivation but their views are often little more than hypotheses rather than conclusions drawn from scientific data These hypotheses must be verified by controlled observations and experiments before they can be accepted as well-grounded scientific theories.

The rapid development in recent times of reliable methods for the controlled observation of infantile behavior in natural-history situations furnishes us with a mass of data regarding the development of child behavior under natural conditions Since most workers in the field of preschool behavior have been interested primarily in the genesis of normal behavior and the determination of age norms, we possess at present only a limited amount of reliable material regarding the natural development of problem tendencies in very young children. Nevertheless, several important studies have appeared which furnish data having a direct bearing upon infantile maladjustments. Recently, Sewall (48) investigated the infantile jealousies of nursery-school and clinic children and noted, among other things, that jealousy was little affected by preparation for the birth of a younger child, that it was dependent upon age differences, and that it tended to decrease with each increase in the number of the family Moreover, jealousies were more pronounced in those families having over-solicitous mothers or inconsistent discipline. Noteworthy was the fact that jealousy among children was more pronounced in poorly-adjusted families than in well-adjusted ones, a condition which may be due to the greater frequency of over-solicitous mothers and inconsistent discipline in maladjusted families Other investigators have corroborated the view that expressions of jealousy in young children are nourished by a background of family maladjustment

The subject of negativism in preschool children has recently been studied by several investigators. For instance, Levy and Tulchin (28) noted that the optimal age levels for resistant behavior were 18 months for females and 30 months in the case of males, while Reynolds (43) and Nelson observed that negativism tends to decrease

with age. Studies of the resistant behavior of young children during test procedure brings out the fact that children are most resistant to those tests involving the child personally, as, for example, imitating the examiner. Goodenough (17), utilizing the Kuhlman-Binet test, observed that boys, as a group, are more resistant than girls and that negativism is most pronounced in boys from the upper occupational classes Tilson (54) suggested that the greater negativism of boys in the higher occupational classes may be due to the greater amount of direct supervision of the child in groups of high socio-economic status Although age level and intelligence are important factors in resistant behavior, it is apparent that environmental factors become increasingly important with increasing age

Evidence for the great importance of environmental influences for the social development of the preschool child is also furnished by the records of nursery schools and hospitals The former, according to Walsh (57), are beneficial, whereas the latter, according to Arlitt and Lloyd (3), succeed with such problems as temper tantrums, feeding, and enuresis, but develop other tendencies, such as exhibitionism and irritability.

A thorough study of preschool difficulties by Tilson brings out the importance of making intercomparisons of a large number of traits having a maladjustmental significance. Her work indicates that intercorrelations among a large number of factors would be extremely important for determining the relative significance of various environmental and symptomatic factors involved in problem behavior Utilizing 225 cases, ranging from one to five years of age, Tilson obtained correlations of 71 between nutrition and need of medical care, 67 between negativism and masturbation, .31 between negativism and restlessness, and —1 0 between restlessness and fear.

Behavior Tendencies of Maladjusted School Children. The significance of maladjustment for the future welfare of the child has led a large number of investigators to give undivided attention to problem children and too little attention to behavior tendencies of the general child population Much needless effort and much fruitless controversy could have been avoided in the past if investigators had compared their results with those from control groups or with previously determined norms of well-adjusted children. However, norms determined for special groups or for the general population are of little value unless statistically reliable and properly validated. Moreover, conclusions which are not based on data obtained through

controlled observation have very little value. Conclusions based upon uncontrolled observation are well illustrated by the diverse opinions held by some leading psychoanalysts regarding the relation between position in the family and social maladjustment. For example, Brill considers maladjustment most pronounced in the only child, Hug-Hellmuth in the case of the middle child, and Adler as regards the first-born Recently a scientific attack upon the same problem has been made by several psychologists Korn (26), working with three groups, that is, 25 only children, 25 oldest, and 25 middle children, found that the only children had the best work habits, and, contrary to common belief, were good mixers The middle children, on the other hand, were more stable than the other two groups. Similar results were noted by Fenton (11) in the case of children ranging from the kindergarten to the sixth grade

At the present time there is a growing tendency among students of child behavior to carry out extensive controlled observational or experimental investigations in order to secure norms of behavior. For instance, Olson (38), in an investigation of nervous habits (tics) in the case of several hundred school children, observed that these maladjustments are distributed normally. In other words, it is quite normal to possess nervous habits Such results tend to call into question the diagnostic procedure of some clinicians who look for nervous habits as reliable indicators of a psychopathic disposition. Olson concludes from this study and others that maladjustment is the problem of every child. A normal distribution of personality traits is also indicated by the findings of Radina (42), who studied 137 children entering kindergarten in Russia and found one-fourth hyperexcitable and one-fourth at the other extreme characterized by lassitude

Probably the most comprehensive and carefully controlled study of character traits in the case of school children was that conducted by May and Hartshorne (19). In this investigation, the behavior of the children was observed in various types of problem situations The groups were chosen from three environmental backgrounds, that is, superior, average economic, and low socio-economic surroundings Only a few of the important results derived from this investigation can be mentioned in this paper It was found, for instance, that such a trait as deceit can be measured with a degree of accuracy comparable to that achieved by many intelligence tests Another important finding was that children are not consistent in their moral

behavior but that it is determined by the nature of the stimulus situation. For instance, in the case of the various tests for deceit, only three children cheated consistently on all of the tests It appears that the dishonest children are suggestible and unable to resist the temporary incentive to cheat aroused by the specific situation. These studies also suggest the tremendous importance of the home environment, since from Grade 7 through high school honesty gradually increases for the group of children from average economic surroundings and gradually decreases in the case of children of poor socio-economic status Moreover, siblings were more alike in honesty than would have been predicted on the basis of their resemblance in intelligence. Furthermore, the effect of classroom morale was apparently an important factor in determining the degree of dishonesty.

Closely related to the character studies of May and Hartshorne and others are the various attempts that have been made to determine the capacity of children to make ethical judgments. Ethical knowledge does not of necessity cause ethical conduct It has been suggested, however, that honest children make use of verbal habits in resisting the temptation to cheat Ability to verbalize in turn depends upon intelligence and chronological age. Meltzer (34), studying 333 children from the fourth grade to high school, found a correlation of +.80 between the understanding of abstract social terms and educational age. In other words, ability to generalize increases with age, and this capacity profoundly affects the nature of ethical judgments. For instance, it was observed that 50% at the 12-year level based their answers to Schaefer's question, "Why is stealing forbidden?" on religious sanction whereas only 10% at the 17-year level made this the basis of their decision

What is a maladjusted child? Problem children are sometimes designated as deviates from the norm. When considered as statistical deviates, variations in a given direction are ordinarily intended. For instance, superior intelligence or a pleasing personality, although deviates from the norm, would not, as a rule, be considered as evidences of maladjustment Apparently what most writers mean when they speak of deviations from the norm or the desired norm is a subjective standard of ideal behavior. It is quite evident that the variability of a given trait may not in itself be made the criterion of maladjustment, since the desirability of a trait is relative to social values. These social standards, as everyone knows, vary with the

age and civilization with which we happen to be dealing. The appraisal of individual behavior, moreover, depends in large part upon the situational point of view of the appraiser. For instance, it is usual to consider as a behavior problem a child who is a consistent trouble-maker, or who makes his behavior a great inconvenience to others, especially to his elders and guardians. On the other hand, an appraiser with a more detached point of view might consider behavior as maladjusted, when it is disadvantageous for the future happiness and welfare of the child. From the latter point of view, such traits as submissiveness and dependence may well be considered as evidences of maladjustment. In an investigation of child behavior and teachers' attitudes, Wickman (61) analyzed problem behavior from these two points of view. On the basis of the ratings of teachers, he noted that maladjustments may be arranged in order of seriousness as follows.

	More serious than		More serious than		More serious than	
Immoralities Dishonesties Transgressions against authority		Violations of Orderliness in classroom Application to school work		Extravagant, aggressive personality and behavior traits		Withdrawing, recessive personality and behavior traits

In contrast with this order, it was found that the ratings of maladjustments on the part of 30 mental-hygiene workers yielded the following results.

	More serious than		More serious than		More serious than	
Withdrawing, recessive personality and behavior traits		Dishonesties Cruelty Temper tantrums Truancy		Immoralities Violations of school work requirements Extravagant behavior traits		Trangressions against authority Violations of orderliness in classroom

It is worthy of note that the mental-hygiene order is practically the reverse of that of the teachers. In other words, teachers favor *submissive* forms of behavior whereas the mental hygienists consider *aggressive* forms of behavior as the more desirable.

Even though we grant that the rating criteria of the mental-hygiene group represent a broader point of view, there still remains the question as to whether certain so-called undesirable traits are

really what they are supposed to be. The importance of certain behavior symptoms posited by some psychiatrists as indicators of maladjustment has not been verified by objective observation. Recently Preston and Shepler (41) compared 17 grade children who had been classed by clinics as needing psychiatric treatment with a normal group and found that, after psychiatric and social investigation of the control group, the latter could not be distinguished from the clinic group. In fact, such behavior as day-dreaming, fears, and defensive lying occurred less in the problem than in the "normal" control group. Another recent study of personality by Conklin (8) yielded somewhat similar results. Conklin made a personality study based upon the psychiatric interview, social case history, etc., of two groups of high-school students, one of which had failed, or were failing, in two or more subjects, the other a control group which was not failing in any subjects and was matched against the maladjusted group for average age, sex, and intelligence. The results of the examination indicate that approximately 50% of both groups present atypical behavior, a fact which suggested to this observer that behavior which we have been considering as evidence of maladjustment may really be quite normal. These reports and others suggest very strongly the need of objective validation of the so-called psychiatric criteria of maladjustment.

Problem behavior and environment The recent development of child guidance clinics has made necessary a change of viewpoint toward the problem of maladjustment Statistical studies have shown that maladjustment is a continuous process extending all the way from mild home problems through truancy tendencies to actual delinquency, involving arrest and conviction by the court The milder forms are receiving emphasis at present due in part to the point of view of mental hygienists and psychiatrists and in part to the interest of psychologists in the development of these unfortunate habits According to a recent survey of Benson[1] and Alteneder, there has been in recent years increasing emphasis placed upon problem behavior in teacher-training institutions throughout the country.

Neither the place in the family nor the size of the family bears an important relation to problem behavior as determined by Levy (29) on the basis of data from 700 problem and 35,000 non-problem children. Many other investigators have arrived at this same con-

[1]See *Ment Hygiene*, 1931, 15, 225-241

clusion in comparing the only child with members of a large family. Ward (59), for example, found that only children exhibited the same type of problems as other children, except that lying, stealing, truancy, and sensitiveness were less frequent, while only children were more restless, were given to crying, nail-biting, and showed school difficulties more frequently

While place in the family *per se* probably exerts little or no effect in the child's adjustment, the attitude of the parents is of immeasurable importance. This has been consistently brought out by investigations regarding the cause of suicides among children (13) and the development of ethical comprehension (35). The attitude of parents and high-school children toward each other was studied at length by Lynd and Lynd (31). Further support of what constitutes a successful parent was determined by Woodhouse (62), who consulted a group regarding their methods. Laws (27) has reduced this problem to a quantitative basis by means of a scale consisting of a battery of four tests

Next to the home, the school is a potent influence in the life of the child Hartshorne and May find that school morale approaches in importance that of the home in building up resistance to the temptation to deceive Rombach (44) studied the effect of the first year of school upon 40 boys. He noted that the first effect of the school situation was to cause children to become too submissive It is noteworthy that until recently lassitude would not generally have been considered a behavior problem. Later, disobedience began, but in its initial stages it was simply misunderstanding of the teachers' directions. Habits thus built up may finally lead to delinquency In this connection it is interesting to note that L. S. Hollingworth (23) reports that even children of very superior intelligence become disciplinary problems due to their tendency toward non-conformity.

Juvenile Delinquency The importance of habits formed by school children is reflected in the many investigations which have been published regarding juvenile offenders. It has been observed that the seriousness of the crime, as measured by the court sentence, differs for the sexes As noted by Healy and Bronner (21), sentences for boys are in the following order from most serious to least serious. stealing, truancy, running away, lying, and sex; for girls, sex, stealing, running away, lying, and truancy A similar relationship was noted by Gruhle in Germany

The age at which problem behavior reaches its height differs for

boys and girls. Ackerson (1), working with 5000 cases at the Illinois Institute for Juvenile Research, found problem behavior to be at its maximum for boys between 9 and 12 years and for girls at 18 years. Healy and Bronner (21) find that the younger children are somewhat more likely to succeed, but the difference between groups from 13 to 18 years and those younger was not great.

As previously suggested in the section on the significance of heredity for maladjustment, until recently the importance of low intelligence in causing delinquency has been overestimated. Anderson in 1921, studying 197 girls from 8 to 19 years, decided that delinquents are about like non-delinquents except that a larger percentage are very inferior in intelligence and a smaller percentage superior. Pintner (40) states that, in general, the percentage of feebleminded among juveniles is greater than among adults and greater among females than males This latter finding is probably due to the fact that females are not committed by the court unless their offences become very serious

One of the most suggestive findings was that of Slawson (51) His results revealed the fact that delinquent boys were better on the Thorndike Non-Language Test than on the National Intelligence Tests, although below the norms for unselected children on both tests They were, however, up to the norm on the Stenquist Mechanical and Assembly Tests Dougherty (9) even finds his delinquent boys superior to the norms. Butcher, Hoey, and McGinnis (6) find the median IQ of delinquents to be 75 and that of their brothers, 86 Their educational quotient is 81 as compared with 93 for the siblings. Here again the problem boys exceeded their brothers in mechanical ability

Since verbal, rather than concrete, intelligence is emphasized in school and this is the type on which the problem boy finds most difficulty, it is not surprising that delinquent tendencies arise Furthermore, delinquent boys have been found to be physically well developed, being as tall as, and slightly heavier than, non-delinquents This tendency toward physical maturity without intellectual and emotional maturity may result in lack of control.

The study of the relationship of emotional characteristics to delinquency has proved a fruitful field Anderson (2) supports Parsons' theory of innate mental abnormality, his data showing nervous or mental abnormality in 52 to 72% of reformatory cases His conclusions have been criticized by Curti on the ground that the

figures included feebleminded, that mental abnormality is not defined, and that there is no evidence as to the hereditary character of the behavior. Healy and Bronner (21) find from 3 to 5% abnormal. Two per cent of Burt's (5) cases are psychopathic and 9% temperamentally defective, that is, requiring supervision. Slawson (51), using the Woodworth-Mathews questionnaire, states that the delinquents gave an "overwhelming preponderance of psychoneurotic responses." He also noted a tendency toward morbid depression, running away, stealing, and fatigue. Social status and nationality did not affect the findings. Chambers (7) observed that delinquents showed emotional precocity as measured by the Pressey X-O tests. Tjaden's (55) delinquent boys of superior intellect exhibited a tendency toward more worry about pain, sex, and death and also more feelings of persecution than college students.

Up to this point consideration has been given to some of the intellectual and non-intellectual traits which seem to bear significant relation to juvenile offenders. To what extent are these factors the outcome of environmental conditions? A partial answer to this question was provided by an extensive study made by Glueck and Glueck (15). These investigators were primarily interested in a satisfactory basis of paroling or dismissing inmates from reformatories. They attempted to determine which facts have the greatest predictive significance for success. A study of reformatory men after dismissal indicates that habit factors are essential in determining success. Forty-six per cent of those who were good workers in the pre-reformatory period met success after dismissal as compared with 12% success for poor workers. Those rated as irresponsible preceding sentence were 64% total failures, while only 41% of the responsible were total failures after dismissal.

In the last few years psychologists have come to place greater emphasis upon the environmental factors causing delinquency. Surveys have shown a large percentage of the homes in cases of delinquents broken by divorce or death. In a group of 341 girls observed by Mathews (33) this factor was predominant. Fearing (10), studying 88 delinquent boys who were psychopathic, found that two-thirds of them had broken homes as compared with 50% for all delinquents and 25% for the general population. Shideler (50) found that 50% of the delinquents, as against 25% of non-delinquents, come from broken homes. This same condition applies to the delinquents investigated by Burt. Slawson found more than

twice as many delinquents having abnormal parental relations as a group of unselected public-school children These abnormal conditions were greatest among delinquent children of low social status, which seemed to indicate that social status may have some significance in the development of problem children

The effect of broken homes may be due in part to the lack of effective discipline Burt, using a control group, states that defective discipline is the most important of the 15 conditions studied Eighty per cent of his delinquents lacked home control as compared with 11% of the control group Defective family relationships ranked tenth in this list He states that the effect of the home is more important than outside conditions in the environment Healy and Bronner noted that home control was lacking in 40% of the 4000 cases studied. Both Burt and Healy and Bronner discovered excessive quarreling in the homes of their delinquents Burt finds six times as much quarreling in delinquent homes as the control group, while the data of Healy and Bronner show quarreling in 12% of the cases

Actual poverty may account for a certain amount of maladjustment Breckinridge and Abbott (4) found that 38% of the delinquent boys and 69% of the delinquent girls came from poor homes Burt states that 18% of his delinquents were below the margin of bare subsistence while this applied to only 8% of his control group Healy and Bronner found that 20% of the delinquents were very poor. Conditions which ordinarily accompany poverty may be responsible for delinquent behavior. For example, homes in poor neighborhoods are apt to have undesirable recreational facilities, although Burt found no difference between his delinquency and nondelinquency groups. Healy and Bronner found 20% of the cases had poor recreation and Breckinridge and Abbott state that over 50% of the 832 cases were not within accessible distance of parks Shaw (49) recently made a study of the delinquent areas of Chicago which brought out the fact that there were certain localities in the crowded center of the city far from parks which furnish a large percentage of the delinquents. All radii outward from the city center show a consistent decline.

Closely related to the problem of crowded conditions is the formation of gangs. Thrasher, studying over 1300 gangs, found that proximity was one of the most important elements Shaw, studying the records of 6466 unselected cases, found two or more participants

in 91% of the cases of stealing. Healy and Bronner found companionship a causative factor in 62% of the cases Even in the cases of boys of superior intelligence, Tjaden (55) found bad companions a factor in over three-fourths of his 26 delinquents Burt, on the other hand, believes that bad companions play a minor rôle in the development of social maladjustments.

Uncongenial school and employment conditions have also been considered as very important causes of maladjustment, yet the investigations of Healy and Bronner, as well as Burt, show them to be causative factors in only a small percentage of the cases

III. Diagnosis and Treatment of Behavior Problems

Diagnostic Procedure. Diagnostic procedures depend in large part upon the nature of the results obtained and upon the techniques used in acquiring data in the field of behavioral adjustments. The results obtained in the scientific study of child development serve a guiding or a directive function in diagnostic procedure They guide us in so far as they furnish a basis for determining what behavioral phenomena are the most distinctive marks of maladjustment. Moreover, scientific data regarding normal children and deviates from the norm furnish us with the genetic background for behavioral maladjustment The procedure and results of the impartial scientific investigator supply the methodological background for the diagnostic approach. Thus, for example, the development of psychometric techniques, which in their nature are standardized on the basis of actual results, may have a diagnostic significance because of this form of standardization. In other words, the technique can be used for supplying a quantitative picture of the deviates from the desired norm, that is, with respect to deviations which are especially significant for behavioral maladjustment. Thus we see that not only do the results of child research enable us to economize in time because of the knowledge of the developmental factors which are significant, but also provide us with techniques which may be taken over in whole, or modified in part, as diagnostic procedures

Many of the older procedures used in clinics were strikingly crude The modern experimental movement, because of its popularity, has led to systematic revisions of the diagnostic approach to behavior problems. The fundamental notion of control either of the observer or of the situation is being considered more and more

important in the handling of behavioral situations. This is especially true with regard to psycho-clinics and is gradually seeping into those clinics having a psychiatric leadership. A tentative classification of diagnostic procedures follows.

I. Psychometric techniques. Standardized for procedure and results
  A Tests for intellectual traits
    1 General ability. Verbal and non-verbal
    2 Special abilities or aptitudes, such as the Stenquist test of mechanical ability
  B Measures of achievement
    1. Educational and achievement tests Diagnostic and prognostic
  C Tests of non-intellectual traits As a rule, poorly standardized
    1. Measures of emotional instability
      a Self-rating scales, such as the Pressey X-O and the Woodworth-Mathews psychoneurotic inventory
      b Other rating scales (investigator, parental, etc) and composite experimental rating scales, such as the introversion-extroversion scale of Marston (32)
      c Association tests, as, for example, the Kent-Rosanoff
    2 Measures of motivation Examples the interest record of Terman, and Hart's test of social attitudes and interests
    3 Tests of character and temperament
      a. Moral traits, such as the tests of deceit, etc, evolved by Hartshorne and May (19)
      b General temperament Example the Downey group and individual tests of will-temperament
      c Tests of suggestibility, etc Example the Otis test of suggestibility

II Experimental and non-experimental approaches other than the psychometric
  A The interview Crude, psychoanalytic, and systematic.
  B Autobiographical essay Example· the general autobiography utilized by Selling (47). Such essays may be either general or specific
  C Rating scales Self-ratings and ratings by the psychologist, etc. Supplementary scales in conjunction with psychometric procedures
  D Case history
  E Short-sample methods, such as that used by Olson (38) for nervous traits in children
  F Miscellaneous experimental approaches, as, for example, the psychogalvanic reflex experiment, reaction-time, etc

The clinical procedure of the psychologist is distinguished by the use of standardized tests given under controlled conditions Psycho-

metric techniques are standardized both for procedure and on the basis of results, the chief function of which is to provide quantitative measures of the deviation from the norm for various behavior traits. In order that diagnosis may be complete it is necessary to obtain a many-sided picture of the child. Minimum essentials consist of the measurement of intellectual and non-intellectual traits and the educational achievement of the child.

Studies of delinquents have shown them to be inferior to non-delinquents in intelligence although less inferior in non-verbal than in verbal intelligence. The Binet test gives opportunity for the psychologist to observe the behavior of the individual under standardized conditions and other types of information which are of equal value may be recorded at this time. Much insight into the personality and interests of the child is gained by the psychologist who secures the confidence of his subject.

The experienced psychologist in giving the individual examination should be provided with a standard chart of personality traits and should rate the child for these traits on the basis of the conditions of rapport, attitude, and remarks with reference to the various parts of the test. The rating scale should provide for the rating of such traits as perseverance, auto-criticism, and attention. It should also include a subjective rating by the examiner of mood and cooperation. The best type is the graphic scale as used by Laird and Wickman. This scale combines linear rating with phrases serving as reference points for the degrees of the scale Five reference points on the scale are a convenient and a sufficient number for practical work An endeavor should be made to use phrases which are as concrete and objective as possible in order that judges will have the same criteria in mind in reaching their decisions.

In this connection a scale devised by the writer (30) and used at Syracuse State School for Mental Defectives might be mentioned The aim was consistently to select criteria which would be objective and within the range of activities of the judge For example, five points for *average initiative* were recorded when the child "needs no supervision on familar tasks if no emergency arises." Seven points were allowed if the child "needs less supervision than most children," nine points, if she "often notices ways of improving work or little things to be done," ten, if the child is "quick to notice ways of helping others and immediately directs them" Scores below five represented criteria which showed decreasing grades of initiative.

Those with inferior verbal intelligence may suffer maladjustment because they cannot keep up with the class in school work which is based upon ability to handle symbols in the form of words. On the other hand, these children may be much more adept in dealing with concrete situations Such tests as the Pintner-Paterson performance scale, the Porteus mazes, and the Dearborn and Ferguson formboards have considerable diagnostic significance. Picture completion tests, as, for example, Healy I and Healy II, serve purposes beyond the final score used as a basis of comparison with norms, as, for instance, the subject's power of auto-criticism, sustained attention, practical judgment, suggestibility, initiative, and emotionality

Special aptitude tests also have a diagnostic significance and furnish a practical basis for treatment As previously noted, delinquents have been found to be equal to non-delinquents in mechanical ability, a fact which suggests the possibility of successfully directing the child's efforts along other lines The Stenquist mechanical and assembling tests therefore should have a definite place in clinical procedure

Since a large part of the child's day is spent in school, the adjustment here probably affects the child nearly as much as his adjustment to his family The clinical psychologist, therefore, should measure the educational achievement of the child A psychograph, such as that used at Waverly, makes possible a quick comparison of the child's achievement with that which should be expected on the basis of his mental age The accomplishment quotient is a useful means for measuring whether the child is working up to ability. Diagnostic tests will be found useful in determining the cause for errors in arithmetical processes Further study involving factors such as memory span may be necessary in the case of reading disability. Prognostic tests may also be found helpful.

In the study of problem children, non-intellectual tests may often be more significant for diagnosis than the measurement of intellectual traits. As indicated in the outline of diagnostic procedures, several tests have been devised for the measurement of personality and emotional instability, as, for example, the Woodworth-Mathews psychoneurotic inventory. Another self-rating scale which has been used to some extent is the Pressey X-O test, delinquents showing emotional precocity in their reactions The Marston (32) extraversion-introversion scale may prove helpful, especially at certain levels The Kent-Rosanoft association test is interesting as afford-

ing a measure of individual differences on the basis of responses to definite stimulus words.

As previously stated, recent research suggests that motivation is more effective than repetition for the genesis of delinquent habits. Insight into the drives of the individual is important, both for diagnosis and treatment. Spontaneous conversational activities during individual examination often supply suggestive material. Terman's interest record and Hart's test of social attitudes and interests furnish a means of obtaining a quantitative measure of the interests of the patient.

The last decade has seen a significant development in the measurement of character traits. The work of May and Hartshorne is by far the most important objective study of character yet attempted. The battery of tests for deception, generosity, self-control, and cooperation should form a useful part of the clinical psychologist's techniques. Delinquents have been found to be more suggestible and incorrigible than control groups. The Otis tests for suggestibility, the Raubenheimer overstatement tests, and Cady's tests for incorrigibility might be effective diagnostic instruments in the hands of an experienced psychologist.

Outside of the use of psychometric techniques, both experimental and non-experimental approaches have been adopted. The interview is usually an integral part of the procedure of the clinical psychologist The crude interview has been used to a large extent in the past, although the systematic interview would undoubtedly furnish a more reliable and satisfactory approach to the problem of diagnosis. Irwin (24) used this type of interview in her work with truants

In addition to the supplementary scale for Binet examinations, a systematic record of home and school conditions is of great practical importance. Sims's score card for socio-economic status, or Williams' scale for grading neighborhood conditions may be used in this connection

The association method may be utilized in a variety of ways for systematic diagnosis. The free association test may be in the form of continuous sentence association, as practiced by the psychoanalysts, continuous word association, or single-word response as used by Jung and Peterson. It might be possible to use controlled association to advantage with a galvanometer and blood-pressure apparatus as part of the set-up.

The case history method is used by both the psychologist and the psychiatrist This is based on reports to the case worker on the part of parents, teachers, etc. It consists of material derived primarily outside the walls of the clinic, together with information gained from the interview. Tjaden (55) studied the case history before the interview in order to help establish rapport. It seems to the writer that in some instances this might actually interfere with the establishment of cordial relations If the subject feels the examiner knows too much about his affairs he may feel that the examiner is just another prejudiced outsider and thus refuse to become confidential. At any rate, the psychologist should never read the report in the child's presence. The systematic type of case history is undoubtedly more helpful than the crude since the former refers more definitely to pertinent information.

The autobiographical essay may be of considerable value for eliciting certain kinds of information from the child. The purpose here is to draw the child out so that he will let information slip which he has been trying to conceal. It is valuable where the child offers resistance. It may be useful for both diagnosis and treatment The general autobiography, as used by Selling (47), may precede or follow the interview Drawing pictures of the home and family has proved another method of gaining rapport and information regarding undesirable behavior.

Therapeutic Procedure The treatment of the unadjusted child and the delinquent has taken many forms. Punishment was the dominant method until recently. Unhappiness for the wrongdoer was considered necessary both for the sake of example and of rectification A statistical study by Glueck of the present correctional system, as exemplified by the reformatory, has shown that it is effective with adults in only about 10% of the cases. Reeducation of children, on the other hand, met success in about 90% of the cases of normal mentality, according to Healy and Bronner. Reeducation in this instance consisted of placement in a foster home

Methods of treatment may be classified roughly under two headings, namely, change of the stimulus situation and change of motivation Under the first we may put hospitalization, institutionalization, and placement in a foster home The change of environment often brings about a sudden removal of the general or specific stimulus situations which elicit the problem behavior The new environment may also motivate the child in beneficial ways Motivation has been shown to

be more important than repetition in the breaking-down of delinquent habits. Wannamaker (58) utilizes play activities of problem children to bring about improvement. Her children were placed in situations where success was assured Truancy may be handled in the same manner The boy who is a failure in the verbal type of school work may enjoy the shop class where he has ability equal to his classmates. Since truancy is often a forerunner of delinquency it should be possible to avert the latter through an appropriate and early treatment of truancy. A quite different type of treatment, which indirectly changes the motivation, is the autobiographical therapy of Shaw. He found that as a result of this treatment his subject "Stanley" gained insight into his motives, a fact which enabled the individual gradually to master his impulses

In reviewing modern trends in the psychology of maladjusted school children, we feel confident that the future will bring more outstanding developments in the practical field of diagnosis and therapy Furthermore, the rapid development of controlled observational methods will undoubtedly be supplemented by increasingly subtle and ingenious methods of handling behavior problems through the experimental approach.

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Association of Consulting Psychologists
38 Eighth Avenue
New York City

LES TENDANCES MODERNES DANS LA PSYCHOLOGIE DES ÉCOLIERS MAL AJUSTÉS

(Résumé)

Une discussion satisfaisante des tendances modernes dans le psychologie du comportement qui offre des problèmes comprend une analyse et une évaluation (1) des méthodes d'acquérir les données, (2) des facteurs causals et génetiques en jeu dans les mauvais ajustments des enfants, et (3) du diagnostic et du traitement des désordres du comportement On présente un résumé détaillé de l'investigation du comportement des enfants, où on fait une distinction claire entre l'approche expérimentale et l'approche non expérimentale. Pendant ce siècle-ci il y a eu un grand accroît des expériences sur le comportement des enfants, mais récemment on est devenu encore interesse aux procédés non expérimentaux à cause d'une conviction de plus en plus grande que les phases importantes du comportement des enfants sont difficiles à attaquer experimentalement. On a fait une investigation des methodes non expérimentales et comme résultat on a revisé les anciens procédés et introduit de nouveaux On a essayé de déterminer à quel point certaines méthodes d'observation contrôlée produisent des notations du comportement non seulement comparables mais valables et cohérentes En même temps que ce mouvement se trouve une attention de plus en plus grande faite aux procedés destinés à neutraliser l'influence des facteurs non contrôlés et ignorés. Dans la discussion de l'origine et des causes des mauvais ajustements des enfants, l'auteur essaie d'indiquer non seulement le changement de l'intérêt pour la nature des problèmes étudiés mais aussi les changements radicaux qui se sont produits dans l'interprétation du comportement des enfants L'auteur montre en conclusion comment les methodes modernes et les résultats de la recherche moderne ont modifié l'approche thérapeutique et présente un résumé détaillé des procedés utilisés à présent pour le diagnostic et le traitement des enfants qui offrent des problèmes

JENKINS

MODERNE RICHTUNGEN IN DER PSYCHOLOGIE SCHWER ANPASSUNGSFÄHIGER SCHULKINDER

(Referat)

Eine angemessene Behandlung der Frage der modernen Richtungen in der Psychologie des unsozialen Betragens [problem behavior) fordert eine Analyse und Bewertung (1) der Methoden zur Sammlung des Materials; (2) der kausalen und genetischen Einwirkungen beim unsozialen Betragen des Kindes, und (3) der Diagnose und der Behandlung der Storungen des Betragens Es wird ein detaillierter Plan zur Untersuchung des Betragens des Kindes vorgelegt, worin eine klare Unterscheidung gemacht wird, zwischen der experimentellen und der nicht-experimentellen Annaherungsweise Wahrend des gegenwartigen Jahrhunderts, hat die Zahl der experimentellen Untersuchungen des Betragens des Kindes stark zugenommen; neulich, aber, interessiert man sich wieder für die nicht-experimentellen Verfahren, weil allmahlich die Überzeigung starker wird, dass wichtige Bestandteile des Betragens des Kindes experimentell schwer anzugreifen sind Die nicht-experimentellen Untersuchungsmethoden sind selber Gegenstande der Untersuchung gewesen, und folglich sind alte Verfahren neu gestaltet und neue eingefuhrt worden Man hat versucht zu bestimmen, bis zu welchem Grade gewisse Methoden der kontrollierten Beobachtung nicht nur vergleichbare sondern auch gultige und zuverlassige Notierungen des Betragens liefern Mit dieser Bewegung geht die zunehmende Aufmerksamkeit einher, die den Verfahren zur Neutralizierung der Einwirkung unkontrollierter und unbekannter Einwirkungen gewidmet wird. In der Besprechung der Genese und der Ursachen der mangelhaften Anpassungsfahigkeit bei Kindern [child maladjustment], versucht die Verfasserin, nicht nur auf die Veranderung in der Richtung der Interesse in Bezug auf die Natur der untersuchten Aufgaben, sondern auch auf die radikalen Änderungen, die in der Deutung des Betragens des Kindes stattgefunden haben, hinzuweisen. Zum Schlusse zeigt die Verfasserin, wie die therapeutische Annäherungsweise durch moderne Methoden und durch die Resultate der modernen Forschung modifiziert worden sind, und liefert einen detaillierten Umriss der gegenwärtig bei der Diagnose und Behandlung schwer anpassungsfahiger Kinder verwendeten Verfahren.

JENKINS

AN EXPERIMENT ON THE ORDER OF ELIMINATION OF BLIND ALLEYS IN MAZE LEARNING*

From the Psychological Laboratories of Clark University

GERARD DE MONTPELLIER[1] [2]

I INTRODUCTION

The temporal order in which the units of a material are learned may be regarded as determined by the degree of difficulty with which the different units are mastered. This difficulty is dependent partly upon the special constitution of the units considered and partly upon other factors, among which great emphasis has recently been placed upon the spatial or temporal distance from the reward or punishment stimulus If this spatial or temporal distance from the goal is an effective factor in maze learning, it is reasonable to assume that this factor will work in such a way as to facilitate the fixation of responses in the part of the maze nearest to the goal in comparison with the responses in the more distant parts. The existence of such a regressive order of learning, shown by a backward order of elimination of errors from the exit to the entrance, would support therefore some form of the law of effect.

This problem has been specifically investigated in many experimental works, and one might undoubtedly get additional data from almost any maze study, and yet the question has not been entirely clarified. Vincent (19), Peterson (14, 15), Carr (4), Husband (12), and Corey (5) conclude in favor of a backward order of blind-alley elimination, whereas Hubbert (6), Hubbert and Lashley (7), Warden (20), and Warden and Cummings (22) do not consider their results as justifying such a conclusion. Recently, K. W Spence (17), reviewing the literature on the subject, has presented supplementary evidence from experiments of Tolman and Honzik, Ruch, Dashiell and Bayroff, and also from the data given by Warden and Cummings, in favor of a regressive order of learning

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[1]Belgian fellow of the C R B. Educational Foundation.

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In the present paper, I shall not discuss in detail the results of the above experiments. On the whole, they seem to indicate that the alleys near the food box are learned first and those near the entrance are learned last. It is to be noted, however, that the data secured in these studies have not all the same validity in regard to the problem in question. In order to show clearly the effect of the goal upon the order of blind-alley elimination, it is theoretically essential that the blinds should be strictly identical in every respect, except in distance from the goal. In the studies listed above, this requirement has not always been observed. The mazes used were ordinarily not homogeneous in regard to the difficulty of the blinds. Indeed, the problem studied has been the order of blind-alley elimination in the particular maze utilized rather than the problem of blind-alley elimination in a maze designed to reveal the effect of the food (or goal) upon the order of elimination. The U-shaped maze used by Warden, Husband, Dashiell, and Bayroff represents a great improvement over the other types, but the most satisfactory is undoubtedly the Warner-Warden symmetrical linear maze. The failure to secure a more perfect correlation between the learning rates for the individual blinds and their distances from the food box may have been due to the failure to equate the blinds in all characteristics except distance from the goal.

In the experiments reported in this paper, an attempt has been made to equate the blinds in the manner just mentioned. With such a maze, the order of elimination has been investigated with both rats and human subjects, and, furthermore, a contribution to the problem has been attempted by studying with rats the order in which errors *reappear* in the complete maze habit when the animal is no longer fed immediately after each trial. The present paper therefore contains data both on the integration and the disintegration of a habit.

II. Experimental Section

Procedure. Four groups of untrained blind rats, 3-4 months old, were run, each group in a different maze pattern. In all cases they were given one trial a day in the maze after having received preliminary training one trial a day for five days in a straightaway path (144 inches long, 4 inches wide, 5 inches high). During the entire working period, the animals were kept in individual cages. When a rat was tested, his individual living-cage provided with food was

placed at the exit of the maze The rat started from an entrance box used by all animals, and so ran the maze to his own living-cage. After reaching the goal, each animal was allowed to eat for about 30 minutes The criterion of mastery was three successive errorless runs.

With rats, a 6-unit diamond maze of the following dimensions was used

total length of the true path · 274 inches
distance between units 14 inches
width of path · 4 inches
height of path 5 inches
length of blind alley (one section) 15 inches

Any one of the two paths of each diamond unit could be closed at any distance from the choice point (by inserting a wooden block between the walls of the path) so that several different patterns could be obtained Retracing doors were placed between the units The top of the maze was entirely covered by a wire mesh

The pattern of Maze I was made by blocking (at the places indicated in Figure 1) the right path of the first, third, and fourth units, the left path of the second, fifth, and sixth units, so that the true path involved, at the points of choice, the following successive turns. left-right-left-left-right-right In this maze, the distance between the units was 8 inches instead of 14, as in the 3 other mazes Seventeen animals were used and learned the maze in an average of 16 trials.

The essential characteristic of Mazes II, III, and IV was that in each case all the blind alleys were on the same side of the central axis The only difference between the patterns was one of complexity In Maze II, the blinds were, as in Maze I, one unit long In Maze III, the blinds were two units long In Maze IV the blinds were as in Maze II, but six more blinds were added as shown in the figure These new blinds pointed away from the general direction of the goal and were entered only a few times Errors made in these blinds are not included in the results shown in the following tables because these blinds were not comparable with those in the other mazes. They were introduced chiefly in order to increase the complexity of the whole situation so as to render the learning more difficult Groups of 17, 13, and 12 untrained blind rats, 3-4 months old, were set to work respectively on Mazes II, III, and IV

For the human subjects, the pattern of Maze I was used as a high-relief wire finger maze [same construction as the multiple-T maze described by Miles (13)]. The total length of the true path was 22 inches, the distance between units, ¾ inch, and the length of blind alleys, 1¼ inch. The maze was placed behind a screen in such a manner that the subject could not see the pattern, but could easily move his finger along the wire. The subject, having placed his finger on the wire in the proper manner, was read the following instructions "When I say 'Go,' move your finger and keep moving always in contact with the wire until you find a nail which will stop your movement You must try to reach the nail by the shortest way. Go as slowly as you wish. Please ask me no questions." As soon

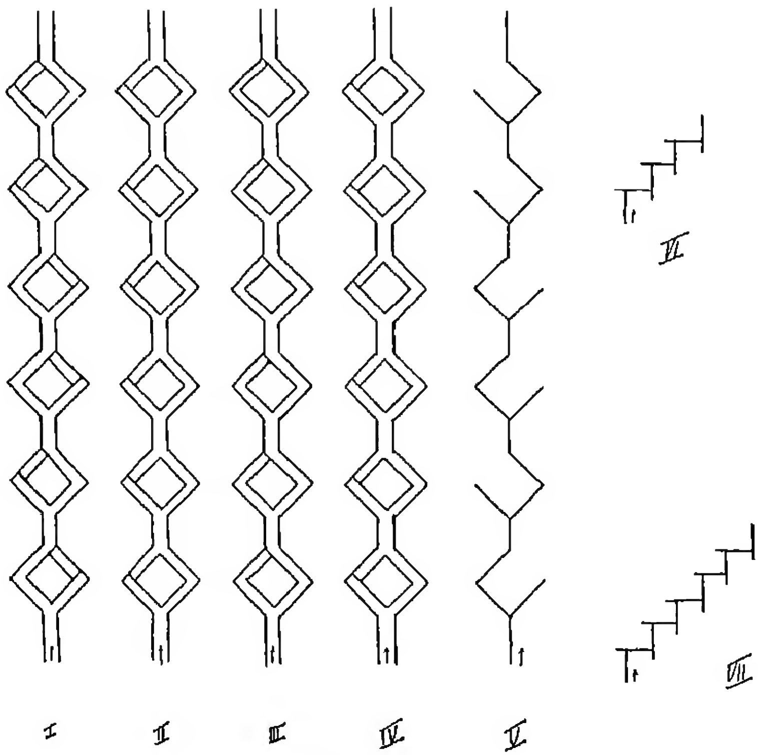

FIGURE 1
THE MAZE PATTERNS

TABLE 1

ORDER OF DIFFICULTY AND TEMPORAL ELIMINATION FOR MAZES I-V

Blinds→	1	2	3	4	5	6
		Maze I				
Errors						
Mean	5.75	3.82	3.41	7.70	2.89	3.35
Median	7	6	5	14	5	5
Trials						
Mean	9.6	8.6	7.2	15.0	7.5	8.8
		Maze II				
Errors						
Mean	4.41	2.59	2.12	1.59	2.59	2.88
Median	5	3	3	2	3	3
Trials:						
Mean	6.3	3.6	3.3	2.3	3.8	4.0
		Maze III				
Errors.						
Mean	2.38	2.16	1.39	1.62	2.00	2.00
Median	4	2	3	3	3	2
Trials						
Mean	3.7	2.8	2.9	2.6	3.1	2.8
		Maze IV				
Errors						
Mean	3.66	3.92	2.84	2.17	2.17	3.08
Median	3.5	5	3	2	2	3
Trials						
Mean	4.6	5.3	3.6	3.0	3.0	4.1
		Maze V				
Errors						
Mean	3.70	9.40	10.60	10.80	6.40	3.05
Median	4.5	14	16.5	14	9	4
Trials						
Mean	7.0	17.6	18.3	18.6	13.9	6.5

as the subject reached the nail, the experimenter said "All right." Before starting on the maze, three trials were made on a wire straightaway path. In all cases, the learning was completed in one sitting

Results

1. *The order of blind elimination during the integration of the maze habit* Table 1 gives, for the five mazes, the average number of errors made in each blind (number 1 is the blind near the entrance number 6 near the exit) and the medians and means of the number of trials necessary to eliminate each one of the blinds. A

blind was considered as eliminated when not entered for at least three successive trials (except in a very few cases where, even after three successive avoidances, the blind was entered many times again, so that it could not be considered as eliminated before that time) and the number of the trial of the last entrance was taken as the point of elimination

As may be seen from the table, the order of difficulty is in all cases closely parallel to the order of temporal elimination For Maze I, blinds 4 and 1 rank respectively first and second, the others being more or less alike. The great difficulty of blind 4 (which appeared in 10 individual cases out of 17) suggested, however, that the pattern used in this maze was not sufficiently reliable for the problem in view. Indeed, although the blinds were similar in shape, length, and disposition in relation to the central axis, so that at each point of choice the blind alley and the true path pointed equally toward the food box, the maze pattern was not strictly homogeneous. beginning as a simple alternation (the first blind pointing to the right, the second to the left), it ended as a double alternation (the third and fourth blinds pointing to the right, the fifth and sixth to the left) This change in the type of alternation is probably one factor responsible for the high number of entrances in blind 4, since this blind marks the first variation in the maze from the simple alternation response However, even a regular alternation in the cul-de-sac succession, as in the Warner-Warden symmetrical maze, for instance, would not be entirely free from all criticism. When the units are disposed along the two sides of a central axis, all the blinds of one side point in the same direction but in a direction which is necessarily different from that of the other group There is no homogeneity in orientation, and therefore the two sets of culs-de-sac must be considered separately. Furthermore, it may be that the last unit of the true path involves a turn in a direction which is much more like the direction of one set of blinds than of the other This is the case in the Warner-Warden maze, as has already been noted by Spence, but it is still more true in the present Maze I, since here the direction of the last true alley is exactly that of all the right-pointing blinds. If the last response before the goal tends to be made earlier and earlier in the maze, as Hunter (9) and Spence (17) have said, then cul-de-sac 4 in Maze I would have an increased difficulty for this reason also

In order to make the culs-de-sac more nearly homogeneous, Mazes

II, III, and IV were devised. As already noted, the essential feature of these new patterns was that all blind alleys pointed in the same direction, so that the two requirements mentioned above were now fulfilled Table 1 shows that for these three mazes the order of learning is practically the same: the part near the entrance is the most difficult; the part near the goal is next in difficulty, and the middle part is in all cases the easiest to master.

The results for the human maze show a situation which is exactly inverse to that obtained with the rats Here, the middle part is the most difficult, the two extremes being much easier, with a slight tendency for the last part to be the easiest one. This primacy-recency order had already been found for human subjects by Warden (20) and Husband (11)

2 *The order of the reappearance of blind entrances during the disintegration of the maze habit* In the previous section, I have presented results showing the order of elimination of culs-de-sac during the mastery, or integration, of the maze habit. This has been the problem attacked by all previous investigators who have thus sought to throw light on the rôle played by the law of effect in maze learning In the present section, I shall report an experiment designed to test in a new way the effect of feeding upon the various parts of the total maze response Blodgett (1) has shown that rats make practically no progress in learning when they are fed one hour after the completion of each trial Where maze learning is progressing normally, with food being given immediately after each trial, the introduction of a delay of three to six hours between completion of a trial and the feeding results, as Bruce (2, 3) and Tolman and Honzik (18) have shown, in a progressive loss of what has been learned. Sharp (16) has demonstrated the same type of disintegration of the complete maze habit when feeding is delayed for two hours. The present experiment is a determination of the order in which the cul-de-sac responses reappear during the disintegration of the completed maze habit, a disintegration which has been produced by lengthening the interval of time between completion of a trial and the receipt of food

After the animals which had run Mazes I and III had reached the criterion of learning, they were given one trial a day during 15 days in the same maze but without food in the end box The rats were fed only some two hours later in their living-cages The results on the distribution of the average number of errors in each

TABLE 2
AVERAGE NUMBER OF ERRORS AFTER REMOVAL OF FOOD

Blinds→	1	2	3	4	5	6
			Maze I			
	2 65	2 77	2 35	2 41	4 35	4 76
			Maze III			
	1 77	2 00	2 16	2 08	3 38	4 46

blind made under these conditions are given in Table 2 It is very striking that in both mazes the number of errors in the last two blinds is much higher than in any other blind Thus in such a disintegration of the maze habit, errors *reappear* chiefly near the exit which is, in general, where they were less apparent during the integration of the habit.

If I were to theorize on the data reviewed and presented in this section, I would do so as follows: The maze habit depends for its establishment, and partially for its maintenance after mastery, upon a certain temporal proximity of the food (or other goal) response to those responses aroused in the maze The maze habit arises fundamentally under the influence of a goal gradient (Hull, 8) which spreads in such a manner that the intensity of the gradient decreases progressively in proportion as the temporal distance from the goal increases If the problem to be mastered, or to be executed after mastery, is too far removed temporally from the goal (food) response, the value of the gradient at that point is so weak that the responses are not integrated in the one case, and in the other case they disintegrate But, in the second case this disintegration would not be equally great in all parts of the maze Indeed, in normal conditions of learning the goal gradient values are different at the various points of the maze, this difference being in direct proportion to the distance between the points considered But, when the food is delayed for two hours, the interval of time between *entrance* to the maze and the food response does not differ from the interval of time between *exit* from the maze and the food response by more than 0 5%, which means that in this situation the gradient values are practically identical in every part of the maze Passing from the first condition (learning) to the second (removal of food), the change in the gradient values will be therefore much greater in the part near the exit than in the rest of the maze, and this change might

FIGURE 2

A SCHEMATIC REPRESENTATION OF GRADIENTS WHEN THE MAZE IS RUN WITH AND WITHOUT FOOD IN THE END BOX

A—goal in the first condition
B—goal in the second condition

well be regarded as a disturbing factor which would be responsible for the higher disintegration appearing in the last part of the maze. This situation is represented schematically in Figure 2, where the gradients *A* and *B* refer to the conditions of running the maze respectively with and without food in the end box (for the construction of the curves see p 134)

III

3 *Reliability of the differences between the various blinds* In order to have some indication concerning the reliability of the differences in difficulty of the various blinds, two methods have been used. The number of errors in each of the six blind alleys has been expressed for each animal, first in terms of a percentage of the total number of errors made throughout the learning, and, secondly, in terms of the rank-order of difficulty (the most-entered blind ranking 6, the next one 5, and so on) From the averages of these percentages and rank-orders of blind entrances, measures of variability have been computed by the ordinary methods The results are summarized in Tables 3 and 4 The means (percentages and ranks) with their standard errors are given for all the mazes The rank-order method was used in order to eliminate the possible disturbing influence of some extreme cases, but, as may be seen from the tables, the parallelism is on the whole very close between the two series of data

The measures of dependability of difference (critical ratio) calculated from these tables, although they are to be taken as only indicative by reason of the small number of cases, give the probable order of difficulty for each maze as represented in Table 5. These orders are based on differences between percentages rather than be-

TABLE 3

Average Percentages of Errors and Variability (Standard Error)

Blinds→	1	2	3	4	5	6
			Maze I			
Learning	21.5	15.2	12.3	27.8	11.3	12.0
	1.81	1.91	1.18	4.44	1.37	1.41
Without food	12.8	13.8	12.6	12.1	21.0	28.0
	2.75	2.10	1.70	1.85	2.45	3.44
			Maze II			
Learning	26.6	16.1	13.5	9.4	16.2	17.2
	2.20	2.07	1.56	1.55	2.06	1.8
			Maze III			
Learning	23.8	16.5	12.3	14.8	16.6	15.9
	3.65	2.67	2.02	3.60	2.04	2.25
Without food	9.1	10.3	12.9	11.4	22.1	34
	1.88	2.76	2.30	2.67	3.00	4.08
			Maze IV			
Learning	19.5	22.2	16.6	11.7	11.3	18.6
	2.65	1.65	2.13	2.45	1.81	2.79

TABLE 4

Average Ranks of Difficulty and Variability (Standard Error)

Blinds→	1	2	3	4	5	6
			Maze I			
Learning	4.8	3.4	2.9	4.5	2.4	2.8
	0.25	0.39	0.27	0.39	0.30	0.30
Without food	2.7	3.2	2.9	2.0	4.5	4.8
	0.42	0.35	0.29	0.29	0.32	0.33
			Maze II			
Learning	5.3	3.5	2.8	2.2	3.3	3.8
	0.22	0.37	0.30	0.28	0.32	0.35
			Maze III			
Learning	4.4	3.6	2.7	3.0	3.6	3.7
	0.45	0.43	0.38	0.46	0.36	0.44
Without food	2.8	2.9	3.0	2.9	4.3	5.0
	0.46	0.39	0.28	0.37	0.29	0.42
			Maze IV			
Learning	3.9	4.8	3.2	2.8	2.4	3.7
	0.50	0.33	0.40	0.50	0.27	0.48
			Maze V			
Learning	2.7	3.9	4.7	4.6	3.3	2.0
	0.30	0.29	0.27	0.34	0.28	0.31

TABLE 5
SIGNIFICANT ORDER OF DIFFICULTY

Order→	First	Second	Third
		Maze I	
Learning	(1-4)	(2)	(3-5-6)
Without food	(6)	(5)	(1-2-3-4)
		Maze II	
Learning	(1)	(2-3-5-6)	(4)
		Maze III	
Learning	(1)	(2-3-4-5-6)	
Without food	(6)	(5)	(1-2-3-4)
		Maze IV	
Learning	(1-2-3-6)	(4-5)	
		Maze V	
Learning	(3-4)	(2-5)	(1-6)

tween rank-orders. In the case of Maze I, blinds 1 and 4 are significantly more difficult than any other, but the difference between them is not significant. Blind 2 ranks second, the difference in difficulty between it and the three other blinds being such that, if not entirely significant, the chances are, however, in every case more than 90% in favor of a difference greater than zero. In the case of Maze II, blinds 1 and 4 are respectively the most and the least difficult, all the others ranging between them at approximately the same level. In all cases, the chances are higher than 96% that this order of difficulty would be true. For Maze III, the only practically significant differences are those between blind 1 and each of the other blinds (the chances of a true difference being in no case less than 94%). Blind 1 would thus rank first, all the others occupying the second place. For Maze IV, the differences between blinds 1, 2, 3, 6 are insignificant, but the chances are higher than 94% that these four blinds would be more difficult than blinds 4 and 5.

In Mazes I and III, when feeding was delayed, the number of entrances in blinds 6 and 5 is significantly greater than that for any other blind. Furthermore, the chances are respectively 95% and 98% that the excess of errors in blind 6 over that in blind 5 would be a true difference.

TABLE 6

Percentages of Errors in the Three Parts of Mazes II, III, and IV and Corresponding Variability (Standard Error)

Blinds→	1–2	3–4	5–6
		Maze II	
	43.6	22.3	33.2
	3.01	2.22	3.07
		Maze III	
	40.0	27.2	32.4
	5.02	4.82	3.78
		Maze VI	
	42.0	28.0	30.0
	2.85	3.00	2.61
Average	41.9	25.8	31.9

For the human maze, the order of primacy-recency indicated in Table 1 may be considered as the real order of learning in the conditions of this experiment.

In Table 6 are given the data for Mazes II, III, and IV divided into three parts composed of two culs-de-sac each. The first thing to be noticed is the great similarity between the results for the three mazes. In all cases, the order of difficulty is clearly the same, this order being very accurately indicated by the general averages given at the bottom of the table. The coefficients of dependability computed from this table show that the first part of the maze may be considered as significantly more difficult than the last two parts. (In the case of Maze III, however, the chances of a true difference between parts 1 and 2 and between parts 1 and 3 are only 96% and 88% respectively.) The middle part is significantly the easiest one in Maze II. For Mazes III and IV, the differences are not significant enough to support the same conclusion, the chances that the middle part is easier than the last being only 80% and 69%, respectively. If we admit that the intensity of the goal attraction at each point in the maze (Hull) is the factor chiefly responsible for the fixation of the habit, we may represent the results obtained in these experiments in a diagram as shown in Figure 3. The successive parts of the mazes are on the abscissae, the height of the ordinates representing the value of the goal attraction at each of these parts.

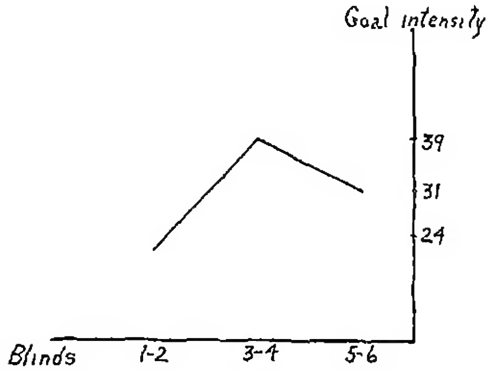

FIGURE 3

Results of the Present Experiment in Relation to the Intensity of the Goal Gradient*

(The goal gradient value at any point is, of course, the inverse of the number of errors made at that point)

IV. Conclusion

In spite of the fact that the order of learning as represented in the general averages of Table 6 is given by each one of the three mazes used, and although, even if the differences are not in all cases entirely significant, the probability would be great in favor of such an order, the conclusion that this order of learning is characteristic of the diamond maze must be taken with caution

A first restricting consideration comes from the fact that the patterns of Mazes II, III, and IV were very easy to master The average number of trials were 7 5 for Maze II, 5.6 for maze III, and 8 for maze IV, so that the total amount of errors was relatively small Furthermore, as already seen, the only thing which seems to be definitely established is the existence of a maximum of difficulty in the first part of the maze under normal conditions of learning, whereas, after the food has been removed, this maximum shifts towards the last part If these results do not fit with a regularly decreasing goal gradient, as a strictly regressive order of blind elimination would, they do mean at least that the goal is exciting an effect upon the distribution of learning

*Intensity of goal gradient—reciprocal of percentages of blind entrances

TABLE 7
AVERAGE NUMBER OF ERRORS IN MAZES VI AND VII

Blinds	1	3	5	7	9	2	4	6	8	10
				Maze VI						
	11.5	7.0	4.5	—	—	5.6	2.2	1.5	—	—
				Maze VII						
	14.7	6.5	3.4	3.3	4.2	7.5	5.3	3.8	2.7	2.4

IV. ELEVATED MAZE OF SIMPLE-ALTERNATION TYPE

In regard to the results secured with these diamond mazes I would present now some data obtained from a group of preliminary experiments in which an elevated multiple T-maze of the simple-alternation type has been utilized.

A 6-unit and a 10-unit elevated maze was run, two trials a day, by two groups of 6 and 14 blind rats (3-4 months old) respectively The dimensions of these mazes were the following

length of each unit of true path. 24 inches
length of blind alleys· 10 inches
width of path· ¾ inch

The average number of errors made in the various blinds are given in Table 7. As the mazes were not provided with retracing doors, some retracings occurred in the beginning of learning The errors of entrances in blind alleys made in such backward runs are not included in the results given in Table 7. As noted in the first part of this paper, the simple-alternation type of maze, taken as a whole, is not homogeneous in regard to the difficulty of the blind alleys But if, for the treatment of the data, the maze is split longitudinally in two halves and the two sets of blinds are considered separately, this objection may be disregarded. In these conditions, as may be seen from Table 7, the order of blind difficulty is for the two mazes almost strictly backward from the goal Some of the differences are probably not significant, but on the whole the regressive order of learning is clearly indicated. This result contrasts partially, therefore, with that obtained for the diamond maze What would be the factor responsible for this difference, the writer is unable to say The fact that, even when the conditions seem the most favorable for the appearance of a regressive order of error elimination, this order is not always clearly obtained stresses the point that the complexity of the maze situation is such as to render

doubtful the possibility of equating completely the influence of all the stimuli which are to play a rôle in the integration of the habit The goal is undoubtedly the prepotent stimulus, but some other factors, not always apparent in the sight of the investigator, may become differential at some place in the maze and more or less mask the influence of the goal factor

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University of Louvain
Louvain, Belgium

UNE EXPÉRIENCE SUR L'ORDRE DE D'ÉLIMINATION DES IMPASSES DANS L'APPRENTISSAGE DU LABYRINTHE

(Résumé)

Une contribution à l'étude du problème de l'ordre d'élimination des erreurs dans l'apprentissage du labyrinthe par le rat blanc a été faite en utilisant deux types de labyrinthe Un premier groupe d'expériences fut fait avec un labyrinthe à parois comprenant 6 unités identiques (impasses et segments de la bonne voie), disposées dans le prolongement l'une de l'autre Dans le second groupe d'expériences, on utilisa un labyrinthe élevé, en *forme de T, réalisant un parcours d'alternance simple* Plusieurs groupes de rats blancs aveugles furent entraînés sur chacun de ces types de labyrinthes

Les résultats obtenus indiquent un ordre d'élimination des impasses pratiquement régressif (à une inversion près) à partir du but, dans le cas du labyrinthe élevé, si l'on considère séparément chacune des deux séries d'impasses de même direction. Dans le cas du labyrinthe à parois, l'ordre d'élimination n'est pas régulièrement régressif les impasses situées dans la dernière partie du labyrinthe sont éliminées après celles du centre, les impasses du début étant comme dans le cas précédent les plus difficiles à éliminer.

Les résultats d'une expérience faite avec un groupe de sujets humains en utilisant un labyrinthe de même dessin que l'un de ceux appris par les rats montrent un ordre de difficulté qui est exactement l'inverse de celui obtenu pour le labyrinthe à parois avec les animaux les deux extrémités du labyrinthe sont ici nettement plus faciles à apprendre que la partie intermédiaire

DE MONTPELLIER

EIN STUDIE ZUM DER REIHENFOLGE DER IRRTUMSVERMEIDUNG IN LABYRINTHLERNEN

(Referat)

Eine Kontribution zum Studie der Reihefolge der Irrtumsvermeidung im Labyrinthlernen wurde mit weissen Ratten und zwei Arten von Labyrinth gemacht. Die erste Reihe Versuche wurde in einem sechsteiligen geschlossenen labyrinth, die andere Reihe in einem erhohten, T-formigen Wechseltyp ausgefuhrt. Durch jede Labyrinthart wurden mehrere gruppen blinder Ratten gebraucht.

Die Resultate zeigten eine fast regressive Reihenfolge der Sackgassenvermeidung im Falle des erhohten Labyrinth, wahrend im anderen, im geschlossenen Labyrinth, die Reihenfolge der Vermeidungen nicht regelmassig regressiv vom Ziel war. Im die letzte Falle, die Sackgassen am Ende des Labyrinth wurden vermieden nach denen im mittelteil, wahrend die nahe des Einganges, wie im vorhergehenden Falle, am schwersten zu vermieden waren.

Die Resultate von Experimenten an einer Gruppe menschlicher Versuchsobjecte—lernen in einem Fingerlabyrinth von einer der an den Versuchstieren gebrauchten Art—zeigten eine Reihefolge der Schwierigkeit welche gerade entgegengesetzt von denen mit Ratten im eingeschlossenen Labyrinth erzielten sind. In diesam Falle, waren Eingang und Ende des Labyrinth viel leichter zu meistern als der Mittelteil.

DE MONTPELLIER

THE EFFECT OF GONADECTOMY AND TESTICULAR TRANSPLANTATION ON HABIT FORMATION AND RETENTION IN THE WHITE RAT*[1]

From The Hunan University, Changsha, China

B. T. Liang

Introduction

The purpose of this investigation is to study the effect of gonadectomy and testicular transplantation upon learning and retention in the white rat. In their study on the effect of ovariectomy on the behavior of young adult female rats, Macht and Dorothy (1) found that the operated rats in maze learning were inferior to the normal ones both in learning time and in efficiency as measured by the number of errors, and that the animals in which only one ovary was removed did not show the same impairment in behavior as the rats with double ovariectomy. Tsai (4) reports that totally castrated adult male rats made consistently poorer records than the semi-castrated animals both in problem box and maze learning, while the records for the latter were poorer than those for the normal rats.

Tuttle and Dykshorn (5) found that when rats were castrated or ovariectomized before 50 days of age, spontaneous activities and the ability to learn a simple maze were not altered before puberty.

From the above studies it appears that if gonadectomy is performed before puberty, learning in the albino rat is not affected whereas post-pubertal operation seems to cause impairment in the learning ability of both male and female rats.

However, (1) in the reports of the above-cited investigations the writers did not analyze their data in sufficient detail to convince the reader that their results and interpretations are unequivocal. It seems, therefore, that a more thorough investigation with a more detailed analysis of results on the effect both on learning and reten-

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[1]I wish to acknowledge my obligation to Professor Z. Y. Kuo for his encouragement and revision of the manuscript, and to Professor C. Tsai for his demonstration of the method of operation.

tion of gonadectomy in rats is necessary before definite conclusions can be reached (2) Furthermore, if learning ability before puberty is not impaired by castration as Tuttle and Dykshorn seem to show, does pre-pubertal castration have any effect on post-pubertal learning? And (3), if there is certain relationship between testis and learning ability, is it due to the functioning of spermatozoa or interstitial cells? This last question has not been investigated, but can be approached by the method of testicular transplantation.

With these three problems in view, we carried out a study which is to be reported in the following pages

Operation and Procedure

In this study 39 male rats and 16 female rats were used Twelve of the males and 8 of the females were gonadectomized between 44 and 52 days of age Autotransplantation was performed on 17 male rats at the same age as just indicated The remaining 10

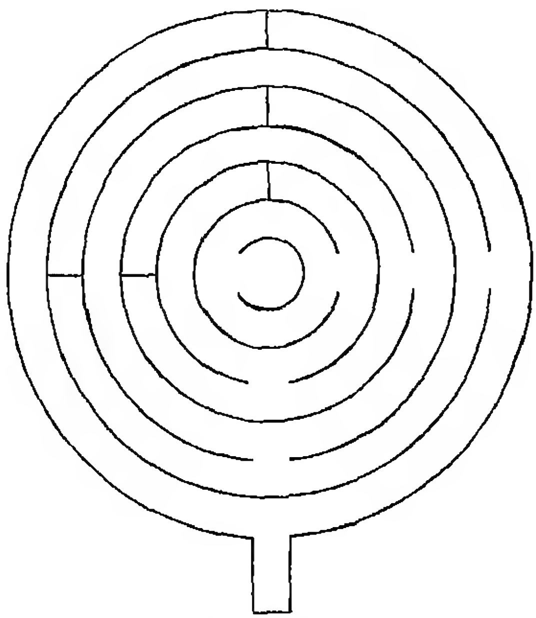

FIGURE 1
Diagram of the Circular Maze

males and 8 females were used as controls. Thus there are five groups of rats: (1) castrated group, (2) transplant male group, (3) spayed female group, (4) male control group, and (5) female control group The usual method of castration was employed Both testicles were carefully removed In ovariectomizing, the abdominal wall was opened and both ovaries removed The gland was then carefully examined to make sure that its removal was complete

In autotransplantation of the testes we used the Steinach technique (2) Both testicles were fixed onto the abdominal muscles The funiculus spermaticus was kept intact, but a week later the connection with the funiculus was cut In the control operation the same technique as performed on the castrated group was followed except that the testes were not removed from the scrotal cavity or otherwise mutilated

Three months after operation all rats were trained in the circular maze (Figure 1) Food was used as incentive Distance traveled by the rat in the maze, time, and number of retracings and errors in each trial were recorded The rat was given one trial every day until it was able to make three consecutive trials without error or retracing and in less than 15 seconds per trial

Twenty days after finishing the learning, each rat was required to relearn the same maze until it reached the former degree of proficiency As before, one trial daily was given

Results

1 *Learning*

a Comparison of the three groups of male rats. The records of the three male groups are given in Tables 1, 2, 3*a*, and 3*b* The numerical data in these tables give the average distance per second for each individual rat, its total errors, retracings, time, and trials The distance per second was computed by dividing the total distance covered by the rat in the maze in the whole course of learning by the total time. Table 4 shows the reliability of the differences between the average numbers of the trials, errors, retracings, time, and distance per second in learning of the three male groups It will be seen from Tables 1, 2, and 3*a* that, so far as the total trials, errors, retracings, and time are concerned, the transplant rats whose transplanted testes were not resorbed are, on the average, better than the castrated group, while the controls made the best records among the three groups There is one exception, however,

namely, that, on the average, the total number of retracings for the transplant group is even smaller than that for the control group. The average number of trials for the controls is 17.2, with a range of 12 to 23, as opposed to 22.1 (range 11-39) for the transplant rats and to 25.5 (range 17-46) for the castrated rats. The average number of total errors of the controls is 45.8, range from 35 to 59, that of the transplant rats, 49.4 (range 29-86), and that of the castrated animals, 63.3 (range 29-117).

The average numbers of total retracings for the three groups are as follows: controls, 19.8 (range 11-28), transplant group, 18.3 (range 10-25), castrated group, 22.0 (range 12-37). The average total time of the controls is 2170.8 seconds (range 1021.4-3853.4), while that of the transplant animals is 2267.8 seconds (range 1413.5-3704.1) and that of the castrated animals is 2369.2 seconds (range 971.2-3436.9). If we compare individual differences among the three groups, the following facts may be noted. Of 12 castrated rats, there is only one animal who masters the maze in fewer trials than the average for the control group; and there are only three castrated rats which made fewer errors than the average control. On the other hand, every control rat made a better record than the average castrated animal in respect to the number of trials and errors. The ratio between the trials for the castrated and control

TABLE 1
LEARNING RECORDS FOR THE CONTROL GROUP OF MALE RATS

Rat number	Total trials	Total errors	Total retracings	Total time (in sec.)	Average distance per sec. (in cm.)
1	16	42	11	1021.4	18.4
2	14	35	19	1701.2	10.8
3	20	40	24	3713.8	10.2
4	14	57	27	3106.7	11.6
5	22	55	18	1539.1	13.6
6	18	46	13	1154.5	12.8
7	12	40	16	1234.2	14.5
8	23	59	28	3853.4	8.7
9	18	39	22	3019.7	12.9
10	15	45	20	1364.3	10.2
Av.	17.2	45.8	19.8	2170.8	12.4
S.D.	3.4	7.9	5.3	1063.7	2.6
P.E.	.7	1.7	1.1	227.1	.6

TABLE 2
LEARNING RECORDS FOR THE CASTRATED GROUP OF MALE RATS

Rat number	Total trials	Total errors	Total retracings	Total time (in sec)	Average distance per sec (in cm)
1	26	89	37	3436.9	10.6
2	25	57	15	2058 0	12 5
3	20	39	31	3428 8	9 1
4	18	50	13	971 2	18 0
5	18	34	16	1351 4	10 7
6	46	117	20	3195 6	15 6
7	27	69	28	3292 3	12 4
8	21	80	30	1545 9	15.3
9	23	51	23	2217 9	11 5
10	17	29	12	1032 1	13 5
11	35	74	24	3184 2	10 2
12	30	70	15	2718 9	13 4
Av	25 5	63 3	22 0	2369 4	12 7
M D	6 1	19.8	6 8	840 3	2 0
S.D	8 0	24 2	7.8	920 1	2.5
P.E	1.6	4 7	1 5	179.2	5

TABLE 3*a*
LEARNING RECORDS FOR 14 TRANSPLANT MALE RATS
The testicular grafts in this group of rats were not resorbed

Rat number	Total trials	Total errors	Total retracings	Total time (in sec)	Average distance per sec (in cm)
1	18	54	20	2536 9	14 1
2	20	41	25	1747 0	15 8
3	39	86	23	2825 6	8 6
4	19	43	12	2260 5	9 0
6	21	29	16	2254 0	10 8
8	27	58	18	2633 0	11 5
9	11	35	14	1656 4	13.7
10	22	50	19	2198 2	13.2
11	16	38	21	1413 5	14 8
12	34	71	24	2748 6	10 0
14	25	32	13	3704.1	10 5
15	19	36	21	2634.2	12 7
16	18	56	20	1706.4	13 8
17	21	62	10	1431 4	15 1
Av	22 1	49 4	18.3	2267 8	12 4
S D	7 0	15 7	4 5	557 2	2 3
P.E	1 3	2 8	8	100 4	4

TABLE 3*b*

LEARNING RECORDS FOR THE THREE TRANSPLANT MALE RATS WHOSE TRANSPLANTED TESTES WERE RESORBED

Rat number	Total trials	Total errors	Total retracings	Total time (in sec.)	Average distance per sec. (in cm.)
1	35	78	18	3291.7	7.5
2	17	36	21	1457.5	18.2
3	18	31	14	2144.9	14.8
Av	23.3	48.3	17.7	2298.0	13.5
S.D.	8.3	21.1	2.9	756.9	4.5
P.E.	3.2	8.2	1.1	294.8	1.7

group is 4.83, and is large enough to insure reliability. The difference in the errors between the castrates and controls is more than three times the probable error, and is sufficiently great to possess considerable statistical significance, for the chances are 98 in 100 that a repetition of this experiment under like conditions would show the castrated group inferior to the controls in the error for maze learning. Between the transplant group and controls the difference is more than three times the probable error only in the case of trials (Table 4). Both in the trials and the errors the critical ratios between castrates and the transplant group are too small to insure reliability. The average differences among the three groups in total time and retracings are not large enough to warrant any positive conclusion inasmuch as there are only small critical ratios, as shown in Table 4.

The difference among the three groups in the speed of running in the maze, as indicated in Tables 1, 2, and 3*a* by the distance per second, is also very small, and it may also be considered as insignificant.

b. Learning process. For the purpose of comparing the three male groups several learning curves for the first 25 trials in each group have been plotted. In Figure 2 is shown the elimination of error in learning of the three groups of male rats. In general, the curves are similar to each other in shape, all showing a rapid initial descent and numerous steeples. But there are some differences which must be noted. (1) The control group's curve for the first trial is higher than those for the other groups, and the transplant group's

TABLE 4

THE RELIABILITY OF THE DIFFERENCES IN LEARNING SCORES AMONG THE THREE GROUPS OF MALE RATS

Measure	Difference*	P E $_{diff}$ †	$\frac{\text{Difference}}{\text{P E}_{diff}}$
Differences between castrated and control groups of male rats			
Trials	8 3	1 72	4 83
Errors	17 5	5.01	3 49
Retracings	2 2	1 90	1 16
Time	198 6	289 50	0 69
Distance per sec	0 3	0.74	0 41
Differences between transplant and control groups of male rats			
Trials	4 9	1 45	3 38
Errors	3.6	3 30	1 09
Retracings	—1 5	1 14	—1 31
Time	97 0	248 33	0.39
Distance per sec	0 0	0 69	0 00
Differences between castrated and transplant groups of male rats			
Trials	3 4	2.01	1 69
Errors	13 9	5 50	2.53
Retracings	3.7	1 72	2 15
Time	101 6	205.64	0 49
Distance per sec	0 3	0 63	0 48

*(*a*) The differences between the castrated group and the controls are obtained by subtracting the average numbers for the controls from the average numbers for the castrated group (*b*) The differences between the castrated and the transplant groups are obtained by subtracting the average numbers for the latter from the average numbers for the former (*c*) The differences between the transplant group and the controls are obtained by subtracting the average numbers for the controls from the average numbers for the transplant group

†P.E $_{diff}$ is computed by the formula $PE_{diff} = (PE_{\cdot 1})^2 + (PE_{\cdot 2})^2$

curve higher than the castrated group's, that is, the controls in the first trial made more errors than the other two groups, and the transplant animals made more errors than the castrated animals (2) The control animals, from the 3rd trial on, eliminated errors much faster than the castrated rats, and, from the 8th trial on, faster than the transplant animals (3) From the 10th trial on, the transplant animals eliminated errors faster than the castrated rats

Figure 3 gives a comparison of the rate of elimination of errors The comparison is made clearer by a translation of the gross values into percentages. The percentage is computed by dividing the value of each trial by the value of the first trial It will be seen in this figure that the control group exhibits the most rapid initial descent in the three groups, the transplant group is more rapid than the castrated group. The percentage for each trial for the control group is smaller than that for either of the other two groups, excepting that for the 7th trial; and that for the transplant animals is smaller than that for the castrated group.

Table 5 shows the time of each trial and the average time of five trials for the three groups In the first trial the controls were inferior to the other two groups After the first trial the controls

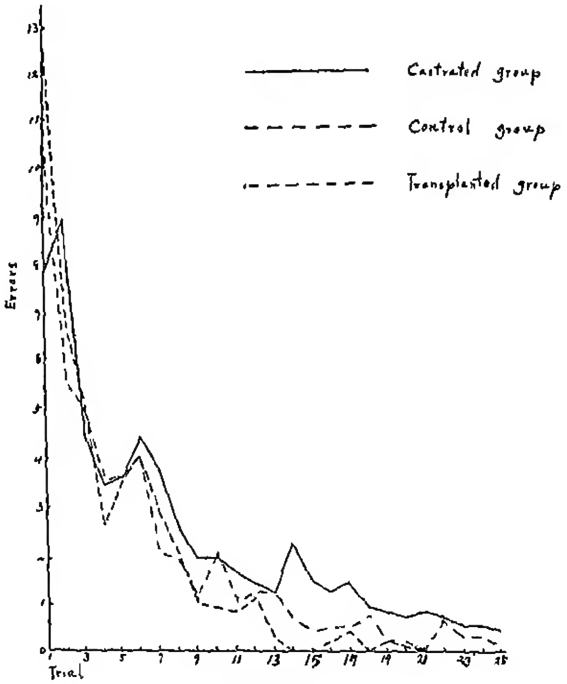

FIGURE 2
CURVES SHOWING ELIMINATION OF ERROR IN LEARNING OF THE THREE GROUPS OF MALE RATS

TABLE 5

TIME OF EACH TRIAL AND AVERAGE TIME OF FIVE TRIALS IN THE LEARNING OF THE THREE GROUPS OF MALE RATS

Number of trial	Control group: Time (in sec.)	Control group: Average time of five trials	Transplant group: Time (in sec.)	Transplant group: Average time of five trials	Castrated group: Time (in sec.)	Castrated group: Average time of five trials
1	948.2		811.6		758.8	
2	521.6		613.6		699.5	
3	151.4	365.18	182.5	369.52	184.3	358.58
4	118.2		120.1		79.6	
5	86.5		119.8		70.7	
6	62.5		68.8		77.3	
7	49.4		55.7		65.4	
8	31.8	39.02	46.3	46.24	38.9	52.08
9	29.2		21.9		49.2	
10	22.2		38.5		29.6	
11	20.8		15.6		31.7	
12	19.5		39.2		32.5	
13	18.6	17.10	31.4	23.68	25.6	27.68
14	17.4		17.9		29.1	
15	9.2		14.3		19.5	
16	9.4		15.8		30.6	
17	19.2		12.4		23.6	
18	7.1	12.34	12.3	12.92	17.8	26.14
19	18.5		11.9		28.6	
20	7.5		12.2		30.1	
21	8.2		9.5		23.2	
22	8.4		19.4		21.8	
23	8.3	8.30	14.3	14.02	24.7	20.48
24			13.5		17.5	
25			13.4		15.2	

TABLE 6

PERCENTAGE FOR AVERAGE TIME OF FIVE TRIALS IN THE LEARNING OF THE THREE GROUPS OF MALE RATS, BASED UPON THE AVERAGE TIME OF FIVE TRIALS FOR THE CONTROL GROUP

	Control group	Transplant group	Castrated group
1	100	101.19	98.18
2	100	118.25	133.47
3	100	138.48	161.87
4	100	104.70	211.83
5	100	168.92	246.74

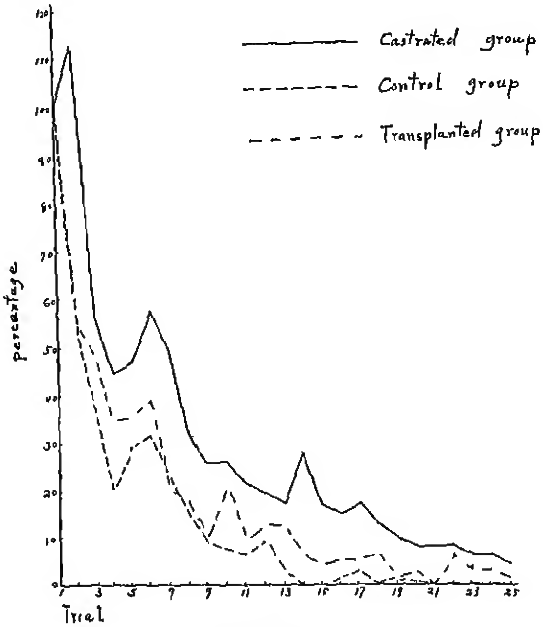

FIGURE 3
CURVES SHOWING THE PERCENTAGE OF ELIMINATION OF ERROR IN LEARNING OF THE THREE GROUPS OF MALE RATS

decreased time faster than the transplant animals, except in the 9th, 17th, and 19th trials. From the 5th trial on, the controls were consistently superior to the castrated animals. From the 13th trial on, the transplant animals decreased time faster than the castrated rats In the first trial, the control group spent more time than the castrated group, so the difference in average time between these two groups is very small For this reason, we think that the number of trials is the best criterion for comparison

In Table 6 a comparison of the average time of five trials in the learning of the three male groups is given. The percentage is computed by dividing each average time of five trials for the transplant and the control group by the average time of five trials for the control group Percentages greater than 100 for either the transplant or the castrated group mean that the particular group spent more time than the control group In the first five trials, the

percentage for the castrated group is the lowest among the three groups. But in the 2nd, 3rd, 4th, and 5th trials, the percentages for the castrated group are the highest The differences of percentage between the castrated group and the controls and between the former and the transplant group are greater and greater as the number of trials increase. This phenomenon seems to show that it is more difficult for the castrated group to make improvement in learning than for the other two groups. The percentage for the

TABLE 7

LEARNING RECORDS FOR THE CONTROL GROUP OF FEMALE RATS

Rat number	Total trials	Total errors	Total retracings	Total time (in sec)	Average distance per sec (in cm)
1	19	36	18	1948 5	12 5
2	23	47	15	2393 2	10 4
3	25	54	11	3076 8	8 4
4	21	63	23	1496.0	14 2
5	34	66	17	3174 4	10.7
6	18	32	10	2238 7	12 2
7	24	51	18	2571 3	13 8
8	26	43	20	2159 8	9 3
Av	23 8	49	16 5	2382 3	11.4
S.D	4 7	11 2	4.1	523 2	2 0
P.E	1 1	2 7	98	125 0	47

TABLE 8

LEARNING RECORDS FOR THE SPAYED FEMALE RATS

Rat number	Total trials	Total errors	Total retracings	Total time (in sec)	Average distance per sec (in cm)
1	22	38	18	1368 5	12.6
2	31	73	26	2985 6	9 8
3	21	56	19	3371.4	10 4
4	11	33	18	2463 6	15 5
5	28	64	27	2832.3	13 2
6	16	32	16	1693 8	10 4
7	23	68	24	2158 4	9 6
8	18	41	17	2024 1	14.8
Av.	21 3	50 6	20.6	2362 2	12.0
S D.	5 8	15 9	4 2	635 2	2 4
P E	1.4	3.8	1.0	151 7	6

TABLE 9

THE RELIABILITY OF THE DIFFERENCE BETWEEN SPAYED AND CONTROL GROUP OF FEMALE RATS IN LEARNING*

	Difference of averages	$P.E._{diff}$	$\frac{\text{Difference}}{P.E._{diff}}$
Trials	—2.5	1 79	—1 40
Errors	1 6	4 64	0 34
Retracings	4 1	1 40	2 93
Time	—19 9	196 55	—0 10
Distance per sec	0 6	0 74	0 81

*The differences are obtained by subtracting the average number for the controls from the average number for the spayed group

TABLE 10

RELEARNING FOR THE THREE MALE GROUPS

Groups	Average total trials	S.D.	Average total errors and retracings	S D	Average total time (in sec)	S D
Control	6 4	1 5	5 4	2 9	106 0	19 3
Transplant	9 3	3 8	7 4	6 5	171 4	106 4
Castrated	11 3	6 0	5.7	4 1	146 7	89 7

transplant group is higher than that for the control group but lower than that for the castrated group, except in the first five trials.

c Comparison of the two female groups. In the female rats only the spayed group and the controls are to be compared. The records of these rats are tabulated in Tables 7 and 8 The reliability of the differences between the averages of the two groups is shown in Table 9. It will be seen that spayed rats were slightly better than the other group in respect to the number of trials, time, and speed of running, but as to the number of errors and retracings the controls appear to have made better records The differences between the two groups, however, when all the cases are taken into consideration and when the data are considered from a statistical point of view, do not seem to be great enough to warrant a conclusion.

2. *Retention* The average results for relearning obtained by the three groups of male rats are presented in Table 10. The differences between average, the P E 's of these differences, and the critical ratios are shown in Table 11 The amount of retention was measured by the saving scores We used three formulae . Formula 1 is the formula of Ebbinghaus The other two are as follows:

Formula 2

$$\text{Saving score} = \left(1 - \frac{\text{Errors made during, or time required for, first 3 trials of relearning}}{\text{Errors made during, or time required for, first 3 trials of learning}}\right) \times 100$$

Formula 3

$$\text{Saving score} = \left(1 - \frac{\text{Errors made during, or time required for, the trials after the 3rd trial of relearning}}{\text{Errors made during, or time required for, the trials after the 3rd trial of learning}}\right) \times 100$$

Formula 2 has been used by Tsai (3) who calls it the method of recall. The saving score obtained by the use of Formula 1 is called saving-score 1; that by Formula 2, saving-score 2, and that by Formula 3, saving-score 3

The results obtained by the three formulae are given in Table 12. From Tables 10, 11, and 12 the following statement may be made:

1. The average number of trials for the controls is less than those for the castrated and the transplant group. The average time for the controls is less than that for the transplant group. The differences between the control and the transplant group in errors and time are insignificant In Table 11 we find that none of the differences between the castrated and the transplant groups is large enough to insure reliability.

2. The total saving scores, that is, the percentage of retention obtained by Formula 1, for the controls are larger than those for the transplant and the castrated groups; but in errors the saving score for the castrated group is the largest. Except in the number

of trials, the saving scores are larger for the castrated than those for the transplant group

3 By Formulae 2 and 3 we obtain two measures, (*a*) one in terms of errors, and (*b*) one in terms of time The two values *in the first three trials, as obtained by Formula 2 (saving-score 2),*

TABLE 11

THE RELIABILITY OF THE DIFFERENCES IN RELEARNING AMONG THE THREE GROUPS OF MALE RATS

Measure	Difference*	PE_{diff}	$\frac{\text{Difference}}{PE_{diff}}$
Differences between castrated and control groups of male rats			
Trials	4 9	1 21	4 04
Errors and retracings	0 3	1 01	0 30
Time	40 7	17 98	2.26
Differences between transplant and control groups of male rats			
Trials	2 9	0 75	3 87
Errors and retracings	2 0	1.32	1 52
Time	65 4	19 67	3 33
Differences between castrated and transplant groups of male rats			
Trials	2 0	1 35	1 48
Errors and retracings	—1 7	1 42	—1.20
Time	—24 7	25.98	—0 95

*The differences are obtained by the method described in the footnote of Table 4

TABLE 12

PERCENTAGES OF SAVING FOR THE THREE MALE GROUPS

Groups	Saving-score 1* Trials	Saving-score 1* Error and retracing	Saving-score 1* Time	Saving-score 2† Error and retracing	Saving-score 2† Time	Saving-score 3‡ Error and retracing	Saving-score 3‡ Time
Control	62 9	91 9	95 1	87 6	95 6	97 3	93 7
Transplant	58 0	89 0	92 5	90 3	95 3	87 9	85.3
Castrated	55 6	93 4	93 8	96 5	97 5	91 4	91 2

*Saving-score 1 is the saving score of total record
†Saving-score 2 is the saving score for the first three trials
‡Saving-score 3 is the saving score for the trials after third trial

TABLE 13

RELEARNING FOR THE TWO FEMALE GROUPS

Groups	Average total trials	S D	Average total errors and retracings	S D	Average total time (in sec)	S.D
Control	9 9	4 0	11 0	5 4	248 2	104 5
Spayed	9.2	4 6	13.5	7 1	203 2	110 2

TABLE 14

THE RELIABILITY OF THE DIFFERENCE BETWEEN SPAYED AND CONTROL GROUP OF FEMALE RATS IN RELEARNING

	Difference*	PE_{diff}	$\frac{\text{Difference}}{PE_{diff}}$
Trials	—0.7	1 46	—0 48
Errors and retracings	2 5	2 13	1 17
Time	—45 0	36 25	—1 24

*The differences are obtained by the method described in Table 11

TABLE 15

PERCENTAGES OF SAVING FOR THE TWO FEMALE GROUPS

Groups	Trials	Saving-score 1 Error and retracing	Saving-score 1 Time	Saving-score 2 Error and retracing	Saving-score 2 Time	Saving-score 3 Error and retracing	Saving-score 3 Time
Control	57 5	83 2	89 6	81 6	91 8	88 7	84 1
Spayed	56.9	81 0	91 4	83 6	91 6	78 4	90 9

for the castrated rats are larger than those for the transplant rats. The percentage of errors and retracings for the transplant rats is larger than that for controls In the percentage of time there is only a slight difference between the transplant rats and the controls.

4 The saving-score 3 (the saving score after the third trial) for the transplant rats is lower than that for the castrated rats; and that for the castrated rats lower than that for the controls.

The average results for relearning and the percentage of retention for the two groups of female rats are tabulated in Tables 13, 14, and

15 It will be seen that there is no significant difference between the controls and the spayed rats.

Examination of the Grafted Testes

Six months after the operation all the rats which had finished the retention test were anaesthetized. The testes of the controls were cut off and weighed. The castrated rats were examined to make sure that the removal of the testes had been complete The grafted area of the testicular-transplant rats was excised After removal, the transplants were cleaned oft the muscle and fat surrounding them, weighed by balance, and fixed in Bouin's fluid. They were then dehydrated, embedded in paraffin, and sectioned by 20 micra The sections were stained in hematoxylin and eosin Unmistakable testicular tissues are seen in 14 of the 17 rats. The weights of the transplants are tabulated in Table 16 in which the learning rank according to the number of trials and the weight rank of the transplants are also shown Some of the sections from the grafts of rats Nos 9, 2, 8, 3, 6, 10, 1, and 7 are shown in Figures 4-11, respectively These photographs are magnified to 90 times the actual size in the preparations In Figures 4-10 no spermatozoa can be found in the seminiferous tubules. The interstitial cell was hypertrophied It seems that, in general, there is some relation between interstitial cells and learning ability if we accept the averages of learning records of the three groups of male rats as valid. But the weight of the transplanted testes and the degree of hypertrophy of interstitial cells do not seem to bear any definite relation to learning ability, as the following cases will show Transplant rats Nos. 3 and 6 made poorer records in learning than transplant rats Nos 9 and 2, but the weight rank of the transplanted testes and the degree of hypertrophy of interstitial cells are higher in rats Nos 3 and 6 (Figures 7 and 8) than in rats 9 and 2 (Figures 4 and 5) The learning rank of rat No 1 is very high, but its transplanted testes show some degree of degeneration (Figure 10)

The transplanted testes of rats No 5, 7, and 13 have been resorbed, leaving only part of the epididymis (Figure 11), but the average results of learning of these rats (Table 3*b*) seem to be not worse than that of those whose grafts were not resorbed We seem to be confronted with a difficult question which we cannot explain in the light of the data we have on hand On the one hand, the interstitial cell seems to have nothing to do with learning, for the

TABLE 16

WEIGHT OF THE TRANSPLANTED TESTES (IN GRAMS), WEIGHT RANK*, AND LEARNING RANK† FOR TRANSPLANT RATS

Rat number	Weight of transplanted testes (in grams)	Weight rank	Learning rank
1	.24	12 0	3 5
2	41	6.5	7 0
3	52	2.0	14 0
4	.21	13 0	5.5
6	45	4.0	8 5
8	.38	8 0	12 0
9	.44	5 0	1 0
10	37	9 0	10 0
11	.27	11 0	2 0
12	56	1 0	13 0
14	.41	6 5	11.0
15	.19	14 0	5.5
16	36	10 0	3 5
17	.47	3 0	8 5
Av	.377		

*Weight rank is based on the rank order of the weights of the transplanted testes

†Learning rank is the rank of learning ability based upon the number of trials

rats with grafts resorbed made no worse records than the rats with grafts intact Further, there is no correlation between the weight and size of the grafts and the ability to learn the maze But, on the other hand, if we assume that the interstitial cell has no relation with the learning ability, it cannot be explained why the controls are decisively better than the castrated rats in maze learning However, it may be that in rats Nos 5, 7, and 13 the grafts had not been resorbed during learning but were resorbed after the test. In this connection it must be noted that the seminal vesicles, prostate gland, and penis in these rats were more developed than in the castrated rats But as to when the grafts were resorbed we cannot decide. And it may be that the difference of the weight of the grafts among the transplant rats is not large enough to affect the learning record. The greatest difference in Table 16 is 0 37 grams (0 56—0.19); this is very small when compared with the difference of the weight of the testes between the transplant and the control rats, for the average weight of the testes of controls at that time is 2 61

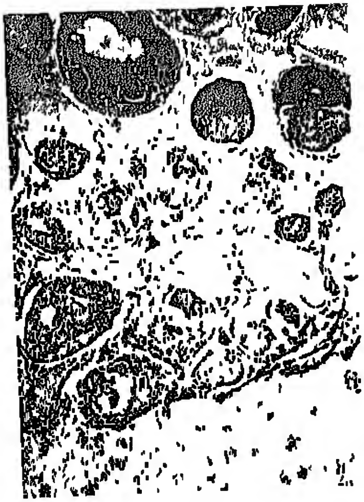

FIGURE 4

FIGURE 5

FIGURE 6

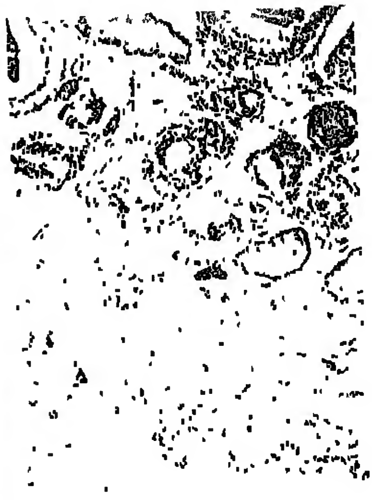

FIGURE 7

PHOTOMICROGRAPHS OF CROSS-SECTIONS OF AUTO-TRANSPLANTED TESTES FROM RATS 9, 2, 8, AND 3, RESPECTIVELY
Magnification x 90

FIGURE 8

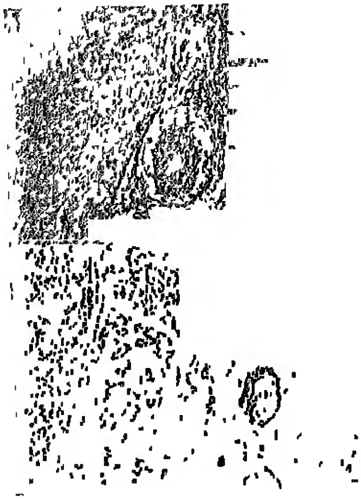

FIGURE 9

FIGURE 10

FIGURE 11

Photomicrographs of Cross-Sections of Auto-transplanted Testis from Rats 6, 10, 1, and 7, Respectively

Magnification × 90

grams, and the difference between those of the transplant and the control rats is from about 2.0 to 2.4 grams.

In a similar manner we may explain why the difference in learning records of the castrated and the transplant groups was very small. But the whole question on the relation between the weight and interstitial cells of the testes and learning ability requires further and more extensive study.

Summary and Conclusion

Thirty-nine male rats and 16 female rats, divided into five groups, were used in this study. Autotransplantation of testes was performed on 17 males, testicular castration on 12 males, and ovariectomy on 8 females The rest of the male and female rats were used as controls Three months after each operation, the rats were trained in the circular maze Twenty days after finishing the learning, retention was tested. Six months after transplantation, all the testicular grafts were excised, weighed, and examined under microscope The results of this study may be summarized as follows:

1 *Learning*

a On the average, the controls made the best records But the differences in learning ability between the operated rats and controls are not so striking as those reported by Tsai Our own results also do not agree with those of Tuttle and Dykshorn who found that there was no difference between controls and operated young male rats in maze learning.

The transplant rats were only slightly superior to the castrated rats in learning It may be that it is not only the quality but the quantity of testes that affect learning ability The size of the grafts remaining in the transplant rats may not be large enough to cause decided superiority in learning over the castrated rats

b. On the first trial, the controls made poorer records than the transplant rats, and the latter poorer than the castrated rats

c Before the fifth trial, there was very little difference among the three groups of male rats

d The controls, after the fifth trial, and the transplant rats, after the tenth trial, were superior to the castrated rats.

e. There was no difference in the speed of running in the maze among the three groups of male rats

f There was no important difference between the controls and

the spayed rats. This agrees with the result of Tuttle and Dykshorn, but is at variance with that of Macht and Dorothy.

2 *Relearning*

a. In the male rats, the total saving scores in trials and time for the controls are the largest among the three groups.

b On the first three trials of relearning the male controls made poorer records than the castrated ones but they made the best records after the third trial It shows that the controls forgot rapidly, but also relearned readily.

c The castrated rats made the best records on the first three trials, but after this trial their records were poorer than that of the controls. It seems that the castrated rats retained the habit most permanently, but found it more difficult to relearn.

d The transplant rats are inferior to the controls There is no difference between castrated and transplant rats in relearning.

It appears that castration seems to have made the rats more phlegmatic and thus made them less disturbed by the new or long-separated situations than the controls Consequently, the castrated rats made fewer errors and spent less time in the first few trials in learning and relearning. But the rate of improvement in the castrated rats was slower than that in the controls, so the number of trials in learning and relearning for the castrated was greater than that for the control group

e In female rats there was also no apparent difference between the controls and the spayed animals in relearning and in the amount of retention.

3. We may tentatively conclude that castration and transplantation in male rats before puberty tends to alter the ability to learn a maze after puberty, but ovariectomy does not show such an effect in female rats.

4. Learning rank was not correlated with histological differences and the weights of the grafts However, since the number of rats used is small, we shall not venture any conclusion on this point

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The Hunan University
Changsha, China

L'EFFET DE LA GONADECTOMIE ET DE LA TRANSPLANTATION TESTICULAIRE SUR LA FORMATION DES HABITUDES ET LA RÉTENTION CHEZ LE RAT BLANC

(Résumé)

On a fait cette étude dans le but d'étudier l'effet de la gonadectomie et de la transplantation testiculaire sur l'apprentissage et la rétention chez le rat blanc. On a employé trente-neuf rats mâles et seize rats femelles On a fait une autotransplantation testiculaire sur 17 mâles On a gonadectomisé 12 mâles et 8 femelles On a employé comme contrôles autres rats mâles et femelles sans ablation

Trois mois après chaque opération, les rats ont été entraînés dans le labyrinthe circulaire Vingt jours après la complétion de l'apprentissage, on a testé la rétention Les mâles de contrôle ont fait en moyenne les meilleures exécutions dans les épreuves et les erreurs parmi les trois groupes de rats mâles dans l'apprentissage Mais dans la première épreuve, le groupe des châtrés a fait les meilleures exécutions dans les erreurs et le temps, les rats de contrôle ont fait les pires Après la cinquième épreuve, les rats de contrôle ont éte supérieurs aux châtrés Dans le reapprentissage, les rats de contrôle ont été en général les meilleurs des trois groupes de mâles Mais dans les trois premières épreuves, les rats châtrés ont donné le meilleur résultat Après la troisième épreuve, les rats de contrôle ont gagné le meilleur résultat d'économie

Les transplantés sont un peu supérieurs aux châtrés dans l'apprentissage, mais la différence n'est pas significante

On ne peut trouver aucune différence entre les rats mâles transplantés et les châtrés dans le réapprentissage

Les mâles de contrôle sont supérieurs au groupe de rats mâles transplantés, mais la différence entre les rats transplantés et ceux de contrôle est petite et moins constante que celle entre les châtrés et ceux de contrôle

Entre les deux groupes de femelles, il ne se montre aucune différence significante dans l'apprentissage et le réapprentissage

Six mois après l'opération, les testicules greffes ont été pesés et examinés sous le microscope Il n'y a eu aucune relation entre le poids et la cellule interstitielle des greffes et la capacité d'apprendre. Il se peut que les greffes qui restent soient trop petites pour influencer la capacité d'apprendre

LIANG

DIE EINWIRKUNG DER GONADEKTOMIE UND DER HODENVERPFLANZUNG AUF BILDUNG UND RETENTION DER GEWOHNHEITEN BEI WEISSEN RATTEN

(Referat)

Es war die Absicht dieser Forschung, die Einwirkung der Gonadektomie und der Hodenverpflanzung auf Lernfahigkeit und Gedachtnis bei der weissen Ratte zu untersuchen Es wurden 39 mannliche und 16 weibliche Ratten verwendet Es wurde an 17 mannlichen Ratten Auto-verpflanzung der Hoden ausgefuhrt Zwolf mannliche und 8 weibliche Tiere wurden gonadektomisiert Die ubrigen mannlichen und weiblichen Ratten, wurden, nach einer Schein-Kastrierung [sham castration], als Kontrolltiere verwendet

Drei Monaten nach der Operation wurden die Ratten in einem runden Labyrinth dressiert Zwanzig Tage nachdem sie mit dem Lernen fertig waren, wurde die Retention gepruft Die mannlichen Kontrolltiere erzielten durchschnittlich die besten Leistungen in Bezug auf die Zahlen der Versuche und Fehler, unter den drei Gruppen der mannlichen Ratten, beim Lernen Im ersten Versuch, aber, erzielte die Gruppe der kastrierten Ratten die besten Leistungen in Bezug auf Zahl der Fehler und Zeitverwand, und die Kontrolltiere die niedrigsten Nach dem funften Versuch waren die Kontrolltiere den kastrierten Ratten uberlegen Beim Wiederlernen erzielten die Kontrolltiere im Allgemeinen unter den drei Gruppen der mannlichen Ratten die besten Leistungen In den ersten drei Versuchen, aber, erzielte die Gruppe der kastierten mannlichen Tiere die beste Leistung Nach dem dritten Versuch erzielten die Kontrolltiere die hochste Leistung in Bezug auf Ersparnis [highest saving score]

Die Gruppe mit der Hodenverpflanzung sind der Gruppe der Kastrierten im Lernen etwas uberlegen, der Unterschied ist aber nicht bedeutend

Es zeigte sich beim Wiederlernen kein Unterschied zwischen der Gruppe der kastrierten mannlichen Ratten und der der Ratten mit verpflanzten Hoden.

Die mannlichen Kontrolltiere sind der Gruppe der mannlichen Ratten mit Verpflanzungen uberlegen, aber der Unterschied zwischen den Ratten mit Verpflanzungen und den Kontrolltieren ist klein und nicht so zuverlassig wie der zwischen den kastrierten und den Kontrolltieren

Zwischen den beiden weiblichen Gruppen zeigte sich kein bedeutender Unterschied in Bezug auf Lernen und Wiederlernen

Sechs Monate nach der Operation wurden die verpflanzten Hoden gewogen und mit dem Mikroskop untersucht. Man fand keinen Zusammenhang zwischen dem Gewicht und den Zwischenzellen [interstitial cell] der Verpflanzung einerseits und Lernfahigkeit andererseits Es kann sein, dass die ubrig-bleibenden Verpflanzungen zu klein sind, um die Lernfahigkeit zu beeinflussen

LIANG

VISUAL PATTERN DISCRIMINATION IN THE MACACUS RHESUS MONKEY*[1]

From the Psychological Laboratories of Clark University

CLAUDE C. NEET

INTRODUCTION

The problem of background, shape, and form in visual pattern discrimination experiments, originally outlined by Hunter (3) and Bingham (2), has been reviewed recently by Munn (5, 7) and Munn and Stiening (8) in connection with experiments on pattern discrimination in the raccoon, the child (Munn and Stiening), and the chick In criticizing Bingham's experiment (1) on pattern discrimination in the chick, Hunter (3) put forth the hypothesis "that all animals below man have only a more or less crude pattern vision and that this probably applies also to a varying period of human childhood" (3, p 330) He pointed out that all visual form discrimination experiments should control the separate aspects of form and background He also suggested a method for this control He maintained that none of the experiments had controlled the background factor and thus the investigations had been of pattern discrimination rather than of form Bingham (2) defined shape and form According to Bingham, "Forms are identical when their areas are equal and their general retinal distribution is similar Shapes are identical when all extensions of the identical forms are equal and in the same retinal directions. Thus, the areas remaining constant, either or both form and shape may change The form remaining constant, the shape may change Change in form must always cause change in shape" (2, p. 140) Munn and Stiening (8) have shown that the background was not essential in a child's discrimination of forms, that the child could respond to either one stimulus or the other regardless of the other, and that the child was able to discriminate form *per se* Munn (7) has shown that the chick may

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[1]This investigation was begun in October, 1931, and completed in April, 1932 The writer is indebted to Dr Walter S Hunter for guidance and encouragement throughout the course of the investigation

respond to only one element of a total situation and that the background of a pattern was not necessarily an influential element in pattern discrimination.

Watson (11) was the first to study the monkey's ability to discriminate visual patterns using a discrimination technique. He found that one of his Macacus rhesus monkeys could discriminate between a circle and a square of the same area. Substituting a square inscribed in the circle caused no change in the response. The animal also learned in 40 trials to discriminate between a hexagon and a triangle. The response was not altered when the triangle was inverted, but, since no other form was substituted for the hexagon, no conclusions could be drawn concerning the problem of shape or form discrimination Johnson (4) compared the visual acuity of a young Cebus monkey with that of human subjects in a well-controlled experiment. He concluded that the monkey's visual acuity compared "favorably with that of the human subjects under similar conditions" (4, p 361).

Other experiments on the visual discrimination of patterns in the primates have been reported. None, however, has been specifically concerned with the problems of background, shape, or form *per se* Furthermore, the majority of these investigations have not been well controlled so that exact knowledge concerning the visual discrimination ability in the infrahuman primates is lacking. Stagner (9) has reviewed the literature on pattern discrimination in the primates

The present experiment is a study of the ability of the Macacus rhesus monkey to discriminate visual patterns The following three problems have been investigated:

1. Is the monkey's discrimination of a pattern determined by the configuration consisting of the figure plus its background, or can the monkey discriminate the figure without the influence of the background? This is the problem suggested by Hunter (3)

2 Is the monkey's discrimination based on the shape of the figure, or can the monkey discriminate the form *per se* of the figure? A response to shape would mean that the discrimination is based on the retinal distribution of light emitted from the figure regardless of the background, while a response to form *per se* would mean that the discrimination is based on triangularity or squareness regardless of the retinal distribution of light emitted by the figure and regardless

of the shape of the background Discrimination of form *per se* is a visual response of the highest type.

3. Is the monkey's discrimination based on the total configuration of positive and negative stimuli or can the monkey respond correctly to a part of the total configuration, i e., to the positive (or negative) stimulus without the influence of the other stimulus? The results of problems 1, 2, and 3 have an important bearing on the *monkey's optical mechanism and are of importance to animal experi*mentation in the field of vision Problems 1 and 3 are of interest in relation to Gestalt perceptual theory

Subjects and Apparatus

The subjects were four Macacus rhesus monkeys. The group consisted of two males, Goof and Bones, approximately five years of age, another male, Squeak, about three years of age, and one female, Sis, approximately five years of age They had all been used in previous experiments in the Clark Laboratory, but none had ever been used in vision experiments The animals were very tame and in good health. The female, Sis, became pregnant about 45 days after the beginning of the experiment Her responses, however, were not noticably disturbed prior to parturition

The apparatus used was a modification of that designed by Munn (6) The principal advantage of this type of apparatus is that the animal responds directly to the stimuli presented Figure 1*a* represents a ground plan of the apparatus

The apparatus, excluding the doors, was constructed of three-ply wall board. It was 30 inches high, covered on the top with one-half-inch wire mesh, and was painted a dull black on the inside A wall-board panel, 7 feet high and 4 feet wide, was constructed at *A* (Figure 1*a*) to screen the experimenter from view of the monkey A 12-by-24-inch opening in this panel at the height of the experimenter's head was covered by three thicknesses of fine wire mesh This served as an effective one-way vision screen through which the experimenter could observe the maneuvers of the animals The electric grid was used for punishment in case of an incorrect response The current for the grid was supplied by a 110-volt A C light circuit shunted through a 0 5-amp , 1750-ohm, "Jagabi" rheostat

The stimuli doors *E* and *E'* were 12 inches wide, 18 inches high, and ¾ inch thick. They were fitted with great care into the

two openings of the back panel so that the inside surface of this panel was perfectly smooth Figure 1*b* shows an isolated stimulus door in relief. A 3/16-inch groove extended across the top width of the door to within 3 inches of the bottom of the door A square, 10 inches on a side, was cut from the front central portion of each door Thus with a white cardboard inserted in the groove, a white

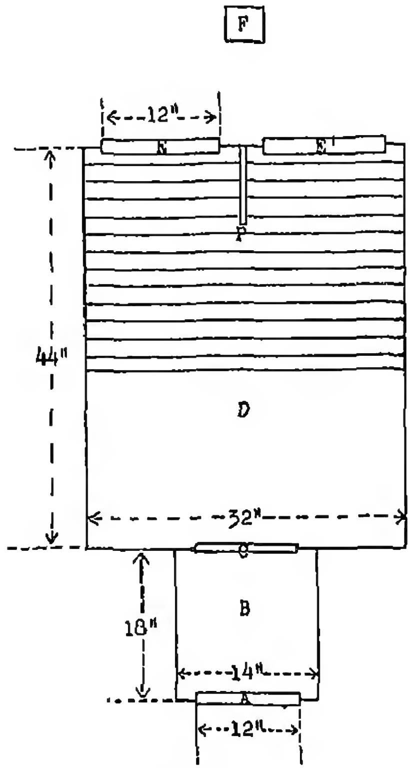

FIGURE 1*a*
GROUND PLAN OF APPARATUS

A is a vertically sliding door which allows entrance to *B*, the entrance chamber *C* is another vertically sliding door which permits entrance to the discrimination chamber *D* The floor of the discrimination chamber *D* is supplied with a shocking grid as shown *P* is a partition extending 9 inches out into the discrimination chamber *E* and *E'* are doors carrying the stimuli to be discriminated The food box, *F*, is situated at a position equidistant from the doors, *E* and *E'*,

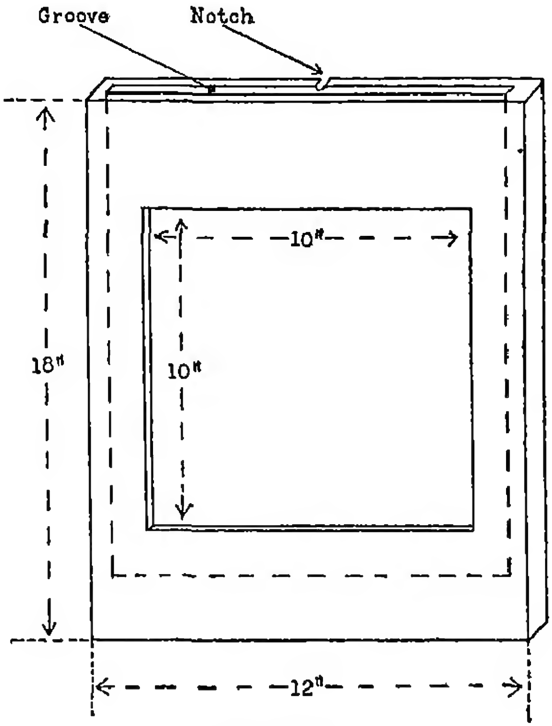

FIGURE 1*b*
STIMULUS DOOR IN RELIEF

square background of 100 square inches was presented. The size of the cardboard was so arranged that no white was ever visible at the top of the doors Small black cords were fastened to the top of each cardboard for removing them from the doors These cords were never visible from the discrimination box; they passed from the groove to the back of the door by way of a notch in the back section of each door Each door was situated an equal distance from the midpoint of the back panel so that the square backgrounds were located in exactly similar positions on each side of the apparatus The stimuli doors, *E* and *E'*, were fastened (each on the edge nearest the center of the apparatus) with spring hinges which caused the doors to swing closed after an animal had passed through Small

metal turnbuttons fastened on the outer door casing served as locks Whenever desired, a given door could be locked to force the animal to pass out of the discrimination chamber by way of the other door.

The apparatus was illuminated by four electric lamps as follows A shaded 15-watt lamp was suspended directly over the entrance chamber at a distance of 30 inches from the floor. The patterns were illuminated by a single, shaded, 50-watt lamp placed equidistant from both the patterns at a point 36 inches in front of the patterns and 30 inches above the floor. The lamp and its shade were tilted at about a 30-degree angle so that the glare of the light would not fall on the monkeys' eyes and so that the stimulus patterns would be evenly illuminated. The food box was illuminated by a shaded, 15-watt electric lamp suspended 36 inches above the floor. This lamp was tilted directly away from the apparatus so that it shed no light in the discrimination chamber A fourth, well-shaded, 15-watt lamp, placed on the front of the experimenter's panel, gave only sufficient light to enable the recording of responses Except for these lights the experimental room was in darkness.

The patterns to be discriminated consisted of black figures appearing on white backgrounds. The stimuli on which the monkeys were trained consisted of (*a*) a black equilateral triangle, placed on its base in the exact center of a white square background, and (*b*) a black circle, of the same area and brightness as the triangle, placed in the center of a white square background. The black equilateral triangle was 6 inches on a side (15 588 square inches in area) for Goof and Bones while the white square background was 100 square inches in area The black circle was the same area as the triangle and its background was also 100 square inches in area The training stimuli for Squeak and Sis differed in area from those of Goof and Bones The black equilateral triangle was 4 inches on a side (6 928 square inches in area) with the circle of the same area. The backgrounds for these areas were each 49 square inches. Except for these area differences, the stimuli for Sis and Squeak were presented exactly as were those for Bones and Goof The smaller areas enabled a rigid control of the area factor in the background controls by making possible an increase or decrease in the area of the square backgrounds. The animals were trained to leave the discrimination chamber by way of the door which bore the pattern consisting of the white square background and black triangular figure This pattern was designated the positive stimulus, while the pattern consist-

ing of the white square background and black circle was designated the negative stimulus.

The black figures which appeared on the white backgrounds were cut from black ticket cardboard They were glued to white, dull-finish, 12-ply cardboard cut to the proper size for insertion in the grooves of the stimulus doors To alter the white backgrounds, for background controls, black ticket cardboard was cut to the same dimensions as the white cardboard so that the two could be slid into the stimulus doors together An opening corresponding to the dimensions and position of the desired white background was then cut out of the black cardboard which was placed in front of the white, so that the white background was limited to the shape and position of the opening in the black cardboard The stimulus cardboards presented smooth, well-fitting, rigid surfaces

Whenever either the backgrounds or the figures were soiled in any manner new stimulus cards were prepared Furthermore, during the control tests, new, clean cards were always used for each animal This eliminated any possibility of extraneous visual or olfactory cues

The entire apparatus was so constructed that all movable parts could be controlled from the experimenter's panel The movable parts were practically noiseless, any noise, however, was constant from trial to trial.

Procedure

The general method of the experiment was as follows. First the subjects were trained to discriminate between (*a*) a black equilateral triangle in the center of a white square background and (*b*) a black circle placed in the center of a white square background After the animals had learned this discrimination, controls were made to determine whether or not the background of the patterns constituted an essential element in the animals' discrimination These controls were carried out by keeping the black figures constant and varying the backgrounds in shape and area. Further controls were then made to determine whether or not the animals' discriminations of the black figures were on a basis of shape or form *per se* These controls were also designed to determine whether or not the animals responded to the total configuration of the two patterns, i e , positively to the positive stimulus and at the same time negatively to the negative stimulus Next a series of tests was made in which the

areas of the normal training stimuli were reduced by one-half on each successive day These controls were used to determine a low threshold area for the discrimination between the triangular and circular figures. Finally, a group of tests was made to determine whether or not the patterns could be discriminated when only portions of the black triangle and circle were presented, i.e., the outlines, sides, base, or apex of the triangle.

The *preliminary training* consisted in getting the monkeys well accustomed to the apparatus and experimental procedure. The animals were brought one at a time from the living-quarters and were taught to go through the apparatus, open the stimulus doors, and pass on out to the food box where they found food Each animal received one grape or a small piece of ripe banana from the food box immediately after each trial. After the animals had thoroughly learned the procedure of going through the apparatus, the regular learning trials were begun. The preliminary training period lasted from six to ten days depending on the animal in question

The specific procedure for all single trials was constant for all animals. The animal was led (by hand at first, later the animal ran ahead of the experimenter) into the experimental room, entered the entrance chamber, and the door was closed behind him. The experimenter went to the back of the apparatus, placed a grape in the food box, locked the negative door, arranged the stimulus cards, and returned to the experimenter's panel at the front of the apparatus The experimenter always passed along the right side of the apparatus, and his movements in arranging the stimuli were always the same at every trial When the experimenter reached the front of the apparatus, he raised the door admitting the animal to the discrimination chamber. (This door was not lowered until the animal had passed through the positive door) The animal then made his choice If he chose the negative door, which was always locked during the learning period, he was shocked just as he touched the door The animal then passed out the positive door and obtained food from the food box. He then returned to the door of the entrance chamber and waited to be admitted for another trial After completing his trials the monkey was returned to his living-quarters where he received the rest of his food

Each animal was given ten trials per day. If an animal went into a position habit during the learning period, the number of trials

was increased and the strength of the shocking current was slightly altered to increase the severity of the punishment. The trials on such a day were continued (still shifting the stimuli according to chance order) until the position habit was broken. The negative door was locked during the entire training period. But after the animals had reached the criterion of mastery this door was never locked.

The criterion of mastery for the discrimination was 9 correct responses out of 10 trials per day for 3 consecutive days. After an animal had reached the criterion he was started on controls. A response at any trial was considered incorrect (or negative) once the animal had touched the negative door with any part of the body. If an animal broke down (made less than 90% correct responses for 10 trials) on any control he was tested with the normal training stimuli until the criterion of mastery was again reached.

The stimuli were presented to the animals in chance order. The amount of food was controlled so that each monkey was well motivated throughout the experiment and the animals were run at a constant hour in the early evening. Further details of the procedure will be discussed in connection with the controls.

Results

1. *Results of Learning Trials.* The results of the learning trials for the four monkeys are presented in Table 1. It is seen that Goof had mastered the discrimination in 140 trials, Bones required 360 trials, Sis, 590 trials, and Squeak, 620. When Goof had reached the criterion of mastery Squeak was seemingly at a point very near mastery. It was at this time that the monkeys had to be moved into new living-quarters and into entirely different surroundings. Moving into the new quarters had a very marked effect on the animals. The first day after being moved Squeak's accuracy dropped to 30%, and it was necessary to test him 46 days more before he reached mastery. Goof's accuracy dropped to 50% on that day but subsequent to this his accuracy rarely fell below 100% and never below 90% while being tested on the normal training stimuli. Except for Goof, who was disturbed by being moved into his new quarters, Squeak was the only animal that ever broke down on the normal training stimuli after reaching the criterion of mastery; this happened after control No. 1.

TABLE 1
RESULTS OF LEARNING TRIALS IN TERMS OF PERCENTAGES CORRECT FOR EACH 50 TRIALS*

Trials	Goof	Bones	Squeak	Sis	Remarks
50	54	50	50	56	
100	80	62	86	52	
150	92 5 (*100-140* trials)	70	88	50	Goof reached criterion of *mastery* on trials 100-140
200		48	46	42	
250		60	46	38	
300		66	46	42	
350		76	46	48	Bones reached criterion of
400		100 (350-360 trials)	58	56	mastery on trials 330-360
450			54	72	
500			56	70	
550			62	70	
600			66	95 (*550-590* trials)	Sis reached criterion of mastery on trials *560-590*
650			95 (600-620 *trials*)		Squeak reached criterion of mastery on trials 590-620

*Final percentages are on basis of 40, 10, 20, and 40 trials for Goof, Bones, Squeak, and Sis, respectively. Criterion of mastery reached in the last 30 trials for each animal.

2. *Results of Background Controls.* After mastering the discrimination, the monkeys were started on the background controls. These controls were presented on consecutive days unless an animal's accuracy on any control dropped below 90% correct for 10 trials. In that case the animal was retested on the normal training stimuli until he had responded with an accuracy of 90 to 100% for 3 consecutive days. When this was accomplished the animal was again presented with the combination which he had failed to discriminate correctly (except control No. 1). If the animal responded correctly to the control on its second presentation, the following day he was tested with the next control on the schedule. However, if he failed on this second presentation he was not tested again with this control for approximately three weeks. In the meantime, training was continued, first with the normal stimuli, and then with the stimuli combinations of other controls. Figure 2 shows the background combinations presented, and Table 2 gives the results for each control.

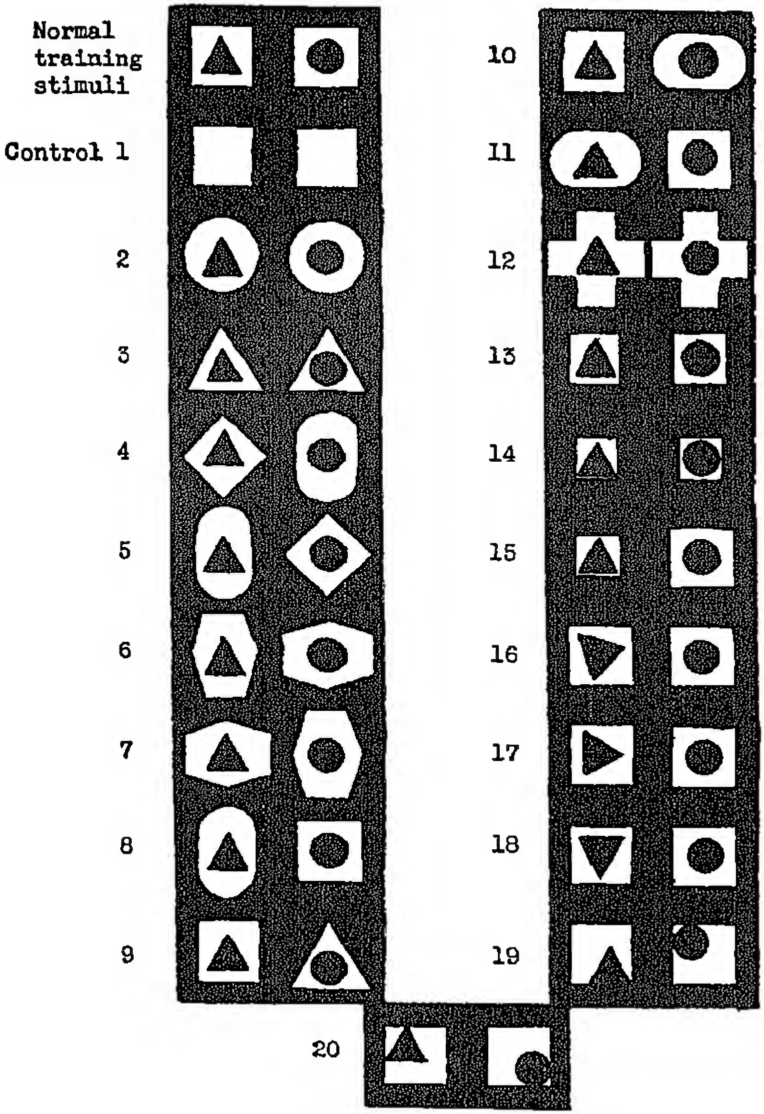

FIGURE 2
BACKGROUND CONTROLS
The pattern containing the triangle was the positive stimulus

Before starting these controls the animals were tested to determine whether or not extraneous cues were influential in the discrimination. This test consisted of presenting two plain white backgrounds without figures (combination 1, Figure 2) Table 2 shows that each of the animals broke down on this control, their accuracy was no better than chance The animals were then put back on the normal series and all excepting Squeak responded with an accuracy of 100% correct for the next 3 days. Squeak was given the normal stimuli for 8 days before he again reached the criterion of mastery. He was then given control No 1 again, his accuracy this time was 40%. His choices for the next 3 days and all subsequent days on the normal series were all 90 to 100% correct This control showed clearly that extraneous cues were not influential in the discrimination

The next control, No. 2, changed the square white backgrounds to circular white ones of the same brightness The background areas for Goof and Bones were each 78 54 square inches (this was 78 54% of the normal area). On this control Goof was disturbed and went into a position habit with an accuracy of 50% On the second presentation he also broke down and it was not until the third presentation that he responded with an accuracy of 100% for 10 trials Bones also broke down on control No. 2; he went into a position habit with an accuracy of 50%, but on repeating the control his responses were 100% correct. The backgrounds (as well as the figure areas) in all controls for Squeak and Sis (except where specifically stated otherwise) were the same area as the normal stimuli. Thus the circular backgrounds in control No 2 for Sis and Squeak were each 49 square inches in area Squeak went into a position habit on control No 2 with an accuracy of 50%; however, on the second presentation he responded with an accuracy of 100% for 10 trials. *On control No. 2 and on every background control Sis responded with an accuracy of 100%*

In control No 3 the normal figures appeared on white, triangular (isosceles) backgrounds The area of the background for Goof and Bones was 50 square inches. Goof went into a position habit with an accuracy of 50%, but on repeating the control (with 3 days of intervening normal series) he made an accuracy of 100% for the 10 trials. On this control Bones responded 9 times correctly out of 10 trials. Squeak, like Goof, broke down on control No 3 but 4 days later he responded with an accuracy of 90% for 10 trials.

TABLE 2

RESULTS OF BACKGROUND CONTROLS*

Control No	Goof	Bones	Squeak	Sis	Remarks
1	40	60	60 40	60	All animals disturbed
2	50† 50‡ 100§	50† 100‡	50† 90‡	100	All animals except Sis disturbed on first test, Goof only on second test
3	50† 100‡	90	50† 90‡	100	Goof and Squeak disturbed on first test
4	100	100	100	100	Not disturbed
5	100	90	100	100	Not disturbed
6	100	90	100	100	Not disturbed
7	100	90	100	100	Not disturbed
8	100	100	100	100	Not disturbed
9	100	100	100	100	Not disturbed
10	100	100	100	100	Not disturbed
11	90	100	100	100	Not disturbed
12	100	100	90	100	Not disturbed
13	100	100	100	100	Not disturbed
14	100	100	100	100	Not disturbed
15	100	100	100	100	Not disturbed
16	100	100	100	100	Not disturbed
17	100	100	100	100	Not disturbed
18	100	not run	not run	not run	Not disturbed
19	100	100	100	100	Not disturbed
20	100	100	100	100	Not disturbed

*Results expressed in percentage correct of ten trials

†First presentation.

‡Second presentation

§Third presentation

Reference to Figure 2 shows that controls 4 to 12 keep the black figures constant but change the backgrounds in various ways. Diamonds, ovals, hexagons, and crosses are used in various combinations The areas of these backgrounds for Goof and Bones varied They were always as large as possible but they were limited in height and width by the 10-by-10-inch square openings in the wooden stimuli doors. However, in each case of controls 4 to 12 the areas for Squeak and Sis were the same as their normal backgrounds. The interesting fact, as shown by Table 2, is that each monkey responded to every one of these controls (4 to 12) with an accuracy of 90 to 100% correct for each 10 trials These results proved that the backgrounds presented in controls 4 to 12 were in no way essential for correct discrimination.

Control 13 reduced the normal white square backgrounds of 100 square inches used by Goof and Bones to squares of 50 square inches in area. Squeak and Sis, naturally, were not tested on this control. Goof and Bones each made an accuracy of 100%.

Controls 14 and 15 were used for the sole purpose of determining whether or not the area of the background was an essential factor for the accuracy of discrimination. These were the first controls in which the background areas were changed for Squeak and Sis. Control 14 reduced the square backgrounds to 36 square inches for Goof and Bones and increased the square background areas for Squeak and Sis to 100 square inches All four monkeys made an accuracy of 100% for 10 trials.

The square backgrounds in control 15 differed in area. For Goof and Bones the small square was 36 square inches in area while the larger was 100 square inches (the normal area). For Squeak and Sis these squares were 25 square inches and 49 square inches in area These backgrounds were not alternated by chance as were the figures. The smaller of the two areas always appeared on the left. All four of the monkeys responded to this control with an accuracy of 100%.

The results of controls 14 and 15 are important. They show conclusively for Squeak and Sis that the area of the background had nothing whatever to do with the accuracy of their discrimination. These controls also show that, at the time they were presented, the background areas were not an influential element in the discriminations of Goof and Bones. As has been stated previously, the background areas for Goof and Bones were not kept equal to the areas of the normal stimuli on which these animals were trained This factor is of importance only in controls 2 and 3, both Goof and Bones broke down on control 2, while Goof alone broke down on control 3 Therefore it cannot be stated positively with regard to controls 2 and 3 that the background was an essential element in the discriminations of Goof and Bones. But in consideration of the results of controls 13, 14, and 15 (which controlled for the area factor) such a statement seems well justified It is conclusive, however, that the background was an essential element in Squeak's discrimination of the patterns in controls 2 and 3.

Controls 16, 17, and 18 consisted merely in turning the triangle through 45 degrees to the left, 90 degrees to the right, and 180 degrees respectively Goof, only, was tested on control 18. Since

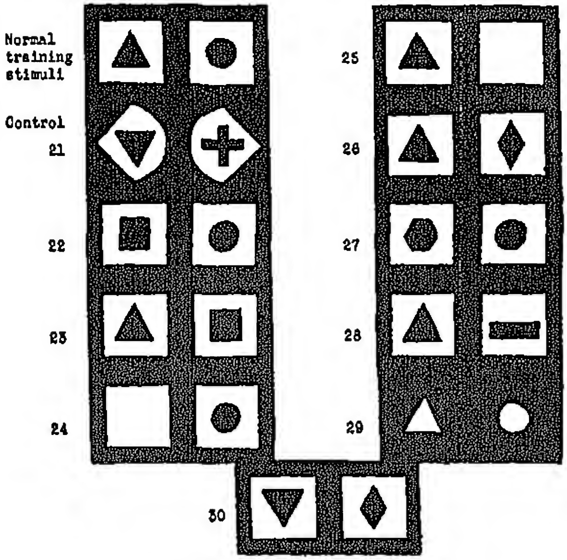

FIGURE 3
CONTROLS FOR SHAPE OF FORM *per se*

later controls used the inverted triangle, it was not used for the other monkeys Furthermore, Bones, Squeak, and Sis were tested on controls 16 and 17 after having been tested on the controls listed in Figure 3. This was done to be sure that the animal would not be conditioned to any different position of the triangle; such changes might have had a bearing on the more important controls of Figure 3 However, all the animals responded to these controls with 100% accuracy. Controls 19 and 20 which also shifted the positions of the triangle and circle were run by all the animals with an accuracy of 100%. Controls 16 to 20 are important in that they show that the position of the triangle and circle in the background is not an essential factor of the discrimination

Summary

a One animal, Sis, ran every background control with an accuracy of 100% for each control. The controls consisted of changes in form of the background, changes in area of the background, and changes in position of the figures appearing on the backgrounds It

can be conclusively said, therefore, that this monkey's discrimination was not determined in any manner by the backgrounds of the patterns discriminated.

b Goof, Bones, and Squeak broke down on control No 2 Goof did not respond correctly to this control until its third presentation. When Bones and Squeak repeated this control their accuracies were 100 and 90% respectively. In control No 3 only Goof and Squeak broke down, but, on repeating the control, they attained accuracies of 100 and 90% respectively The three animals then ran every remaining background control correctly. Controls 14 and 15, in particular, which altered the areas of the backgrounds only, definitely showed that area differences had no effect on the discriminations of Squeak and Sis. Controls 13, 14, and 15 also showed that Goof and Bones were not influenced by area changes in the background. Of course this was true for the period when these controls were given so one cannot say conclusively that Goof and Bones were not influenced by area changes when they were tested on controls 2 and 3 But since Squeak and Sis were not affected by area changes it seems very probable that Goof and Bones were not either Only one difficulty remains. It might have been that if Squeak and Sis had been given their area changes just after mastering the original discrimination they would have been disturbed by different background areas The point is that Goof, Bones, and Squeak, at first, may have been slightly dependent on background factors Two questions arise. (*a*) If Goof, Bones, and Squeak, after reaching mastery, had been tested continually on regular training stimuli until the discrimination had been overlearned would they have broken down when the backgrounds were altered? Or (*b*) did the changes in the backgrounds of the first controls serve to reduce their dependence on any background factors? The present results tend to indicate that, in the case of the three animals which broke down on their first controls, the changes in background had such an effect

c It can be stated conclusively that the backgrounds were an essential element in Squeak's early behavior, and that the same was no doubt true for Goof and Bones. Furthermore, it was shown that these animals soon learned to disregard the backgrounds in their discriminations.

3 *Results of Controls for Shape or Form per se.* After the animals had completed the background tests they were started immediately on controls which substituted various other black figures

for the triangle or the circle. The schedule for presenting these controls was not the same as that for the background controls. The schedule for Goof, Bones, and Sis was as follows: Controls 21, 22, and 23 were presented on consecutive days; then the normal stimuli were presented for one day, and the next day control 24 was given. From this point on, the normal stimuli were presented for three days between each control regardless of whether the animals discriminated the controls correctly or not. Squeak's schedule throughout contained three days' training with normal stimuli between each control test. Figure 3 shows the combinations presented to the animals. In only two cases (combinations Nos. 21 and 29, Figure 3) were the backgrounds altered. In the case of control 21 the background area was 64.27 square inches. Control 29 used only a continuous black background. The white square backgrounds of the training stimuli were used in all other controls of this series. In every case, the black figures appearing on the white background were of the same area and brightness as the figures of the normal training stimuli.

The animals were tested on combinations 21 to 29 (Figure 3) after which they were given controls 31-38. Following these controls, the monkeys were retested on those controls (21-29, Figure 3) in which their accuracies had been below 90% correct for 10 trials. Combination 30 (Figure 3) was presented to Goof and Bones 30 and 31 days respectively after they had been tested on combination No. 26 the second time. Table 3 presents the results of the trials on combinations 21 to 30.

As seen in Figure 3, combination 21 consisted essentially of inverting the triangle (positive stimulus) and changing the circle to a black cross of the same area and brightness. The backgrounds were as shown and remained on a constant side. Table 3 shows that Goof responded correctly to this control. Bones was given 14 trials and made an accuracy of 93%. He made only one error, that on his third trial. Squeak broke down, his accuracy dropping to 40%. It was thought that perhaps the strange backgrounds might have influenced his responses, so, after being presented with the normal stimuli for 3 days, he was given the same control. His responses were 50% correct. Thirty-eight days later he was tested again on combination 21 using the normal square backgrounds. His responses were 60% correct for 10 trials. This showed that Squeak could not respond correctly to this control. Sis responded to com-

bination 21 with an accuracy of 100% for 10 trials. These results show clearly that three of the animals, Goof, Bones, and Sis, could respond positively to the triangle on its apex This may have been a response to form *per se,* for the shape (retinal distribution of light) is altered by inverting the triangle, but there remains a possibility that the monkeys may have been responding to the flat horizontal line of light across the base of the triangle or to the sloping lateral sides This control did show, however, that for Goof, Bones, and Sis the negative stimulus (the cross) was not necessary for the discrimination Squeak's breakdown may have been due either to lack of the normal negative stimulus (the circle) or to the inversion of the triangle.

The monkeys were next tested on combinations 22 and 23 (Figure 3) All four animals responded to combination 22 with an accuracy of 100% for 10 trials, and Goof, Bones, and Squeak responded to No 23 correctly Sis broke down to 60% on control 23. These two controls show quite clearly that all four animals could respond negatively to the negative stimulus (the circle without the influence of the triangle), and, in addition, that Goof, Bones, and Squeak

TABLE 3
RESULTS OF CONTROLS FOR SHAPE OR FORM PER SE*

Control No	Goof	Bones	Squeak	Sis
21	100	93 (14 trials)	40† 50‡ 60§	100
22	100	100	100	100
23	100	100	100	60
24	100	100	100	100
25	40† 0‡	10† 0‡	40	50
26	71† 100‡ (14 trials)	60† 100‡	60† 80‡	60
27	64† 100‡ (14 trials)	50† 90†	93 (14 trials)	86 (14 trials)
28	64† 60‡ (14 trials)	100	50† 50‡	60
29	50	50	50	40
30	90	40	not run	not run

*Results expressed in percentage correct Percentages based on ten trials unless otherwise indicated

†First presentation

‡Second presentation

§Third presentation

could respond positively to the triangle without the circle (negative stimulus) being an essential element There is a possibility that in discriminating the normal stimuli Sis responded positively to the low horizontal distribution of light corresponding to the base of the triangle If this were true then she would have responded likewise to a horizontal line of the square when it was substituted for the triangle. This may have been the case on combination 23 to which she responded with an accuracy of 60% Each animal responded to control 24 with an accuracy of 100%

Control 25 was the opposite of 24 in that it presented the normal positive triangle in the white background against a blank white square background for the negative stimulus. All the animals broke down on this control, Bones responded positively to the blank background 9 out of 10 trials More than 30 days after being tested on control 25 Bones and Goof were retested on the same control. The percentage correct for each was zero, i.e , each responded to the blank white background 10 times out of 10 trials These results show that the monkey responded more readily to the blank, square, white background than to the positive triangle appearing in an "identical" background It is possible that Bones, Goof, and Sis had become conditioned positively to the white square in discriminating combination 24, but this does not hold true for Squeak. He was presented with control 25 before 24 to test this assumption He broke down on combination 25 and later made an accuracy of 100% on control 24 Weidensall (12), working with the rat, and Munn (7), working with the chick, have shown that a white stimulus was much more effective than a black one for setting up a discrimination A similar condition seemed to be present with Goof and Bones. If the black triangle in combination 25 was effective, its effectiveness was not sufficient to prevent a preponderance of responses to the lone white square. This points to the fact that in the discrimination of the normal stimuli (Figure 3), the triangle appearing in the white background was not as effective as the circle on its background, i e , the monkeys could respond positively to the positive stimulus as shown in combinations 21 and 24 but the negative response to the circle on its background was no doubt much more effective in the discrimination

Combination 26 (Figure 3) was next presented This was a crucial control for it would help to determine whether the monkeys could respond to shape or form *per se* The diamond which was

substituted for the negative stimulus was very similar in shape to the triangle, its high and low angles were each 60 degrees For 10 trials Goof's accuracy on this combination was 80% but when given 4 trials more he made two incorrect choices His accuracy for the 14 trials was thus 71%. Bones, Squeak, and Sis all broke down, their accuracies being 60% for 10 trials. However, Goof, Bones, and Squeak were repeated on this control, these repetitions occurred 46, 43, and 42 days respectively after the first presentation (In the meantime the animals were continuing with other controls) At this time Goof and Bones each made an accuracy of 100% for 10 trials while Squeak's percentage was 80 for 10 trials These controls indicate positively that at the time the animals were first presented with this control they could not discriminate on a basis of form *per se* They were no doubt responding to shape, i.e., to the retinal distribution of light The distribution of light from the triangle is very similar to that of the upper half of the diamond, therefore the positive response to the normal positive stimulus (the triangle) must no doubt have been on a basis of shape. On the second presentation of control 26, Goof and Bones may have discriminated on a basis of form *per se*, but no such statement can be made for the triangle was not inverted in this control, changing the shape of the triangle is essential to test a response to form *per se*

Next the animals were presented with combination 27 The fact that Goof and Bones broke down on this control cannot be accounted for unless it is assumed that the retinal distribution of the light of the hexagon was very similar to that of the circle However, Goof, Bones, and Squeak were retested on this control 41, 38, and 34 days (respectively) later This time Goof's accuracy was 100%, Bones' was 90% and Squeak's 100% for 10 trials each This was further evidence that the animals could definitely respond negatively to the negative stimulus without the influence of the positive stimulus.

The next control, combination 28 (Figure 3), was designed to test further the animals' ability to respond positively to the positive stimulus and to help determine whether or not the animal was responding to a low horizontal distribution of light from the triangle Goof's responses were 70% correct for 10 trials, but only 64% correct for 14 trials Nine days later Goof was given 10 trials more. His accuracy in this instance was only 60%. These results tend to show that Goof's discrimination of the triangle in the normal

situation may have been based on the low flat horizontal distribution of light corresponding to its base Bones showed that he could respond definitely to the triangle and that his discrimination was based on some part of the triangle other than its base. Squeak's accuracy was 50% for 10 trials, while that of Sis was 60% Squeak was retested on this control 27 days later but his accuracy was still 50% Squeak and Sis either could not respond positively to the triangle or else their original response to the triangle was based on the low distribution of light emitted by the base of the triangle, which in this case would furnish a basis for discrimination due to the fact that the distribution of light from the rectangle is similar to that emitted by the base of the triangle. Combination 29 was presented to determine whether or not the animals would respond to white figures as well as the black Goof and Bones each went into a position habit with an accuracy of 50%, Squeak's accuracy was no better than chance, while that of Sis dropped to 40% It was clear that neither of these stimuli was more effective than the other. It may have been that the white forms were reacted to as though they were backgrounds They would thus not furnish a basis for discrimination

One more important control must be mentioned in this section. It will be remembered that Goof, Bones, and Sis responded well within the criterion of mastery to control 21, but Squeak's responses for 30 trials were no better than chance. Furthermore, none of the animals was able to respond correctly to combination 26 the first time it was presented The second time this control was presented, Goof and Bones both responded correctly, while Squeak's percentage was at 80 These results tend to show that Goof and Bones, at any rate, might have been able to respond to the form *per se* of the triangle. Therefore, 30 and 31 days respectively after being tested on combination 26 the second time, Goof and Bones were tested on combination 30 It was thought that this was a sufficient delay in case the animals had become conditioned to respond negatively to the diamond the second time it had been presented This combination was the only adequate means by which a response to shape or form *per se* could be tested, for inverting the triangle alters the distribution of light emitted by the triangle, so if the animals responded correctly the only basis of the discrimination would be one of form *per se* In combination 21 the triangle was inverted, but this control had been presented at least 70 days previously so it was very

improbable that any effect from the inversion of the triangle would be retained On control 30, then, Goof responded correctly 9 times out of 10 trials, while Bones' responses were only 40% correct for 10 trials The fact that Goof responded correctly to control 26 on its second presentation firmly substantiates the statement that Goof's response to combination 30 *was on a basis of triangularity or form per se* Bones' results on combination 30 showed that his responses were *not* on a basis of triangularity but were based on shape Goof thus demonstrated his ability to discriminate form in its abstract sense, while Bones was unable to attain such a high level of discrimination

Summary

a The results for Goof and Bones are more definite than those for Squeak and Sis Sis was not retested on any of these controls due to the fact that parturition occurred just before starting the repetitions Goof has shown in four cases (combinations 21, 23, 26, and 30, Figure 3) that he could respond positively to the positive stimulus (the triangle and background) without the influence of the original negative stimulus (circle and its background). He has also shown (controls 22, 24, and 27) that he could respond negatively to the negative stimulus alone. Bones has shown that he could respond in the same manner. His responses to the positive stimulus have been conclusive on combinations 21, 23, 26, and 28, and to the negative stimulus his responses have been conclusive on combinations 22, 24, and 27 Squeak's results are not so conclusive On combination 23, however, he showed definitely that he could respond to the positive stimulus alone and on combinations 22, 24, and 27 that he could respond negatively to the negative stimulus without the influence of the original positive stimulus. Sis, in only one instance, has shown that she could respond positively to the positive stimulus alone, but on combinations, 22, 24, and 27 she has shown without doubt that she could respond to the negative stimulus without the influence of the original positive stimulus Thus Goof and Bones have shown that they could respond as well positively as negatively, and that the relation of the positive to the negative stimulus was not necessary for correct discrimination Squeak and Sis have shown that they, too, could respond positively or negatively. Their responses to the positive stimulus, however, were not as well conditioned as were those of Goof and Bones, but their negative re-

sponses were just as effective as those of the other two animals. The fact that the negative response was so well conditioned is not unexpected, for the punishment connected with an incorrect response in the training period was such as to condition the animal negatively to the negative stimulus before conditioning him positively to the positive stimulus

b. The evidence accumulated by Goof's results on combinations 21, 26, and 30 shows conclusively that he was able to respond to triangularity or form *per se* Bones approached this high level of discrimination as shown by his results on combinations 21 and 26, but in a crucial test (combination 30) he was unable to demonstrate his ability to respond to triangularity. In none of the combinations did Squeak demonstrate his ability to respond to form *per se,* while Sis gave evidence of such ability only in combination 21 This bit of evidence is too inconclusive. If Squeak and Sis had been able to respond more positively to the positive stimulus their responses on combinations 26 and 30 might have shown some evidence of a response to triangularity But since their reactions seemed to be largely based on responding negatively to the negative stimulus, they could not be adequately tested for response to triangularity

4 *Results for Decreasing Area of Stimuli* After each animal had been presented with combination 29 (Figure 3) for the first time he was started on a series of controls testing his discriminative ability when the areas of the black triangle and the black circle were reduced. The background areas were not altered in any way The stimuli for Goof and Bones were first reduced from their normal areas (15 588 square inches) to those used for Squeak and Sis (6.928 square inches). After this reduction, the areas for each animal were reduced by one-half on each day until a point was reached at which the animal's discrimination broke down. When this occurred the animal was again tested for three days with the areas which were twice the size of those on which he had broken down This was done to be sure that he could respond correctly (within the criterion of mastery) to these former areas The animals always responded correctly in these situations Then the animal was retested with the areas on which his discrimination had last broken down If he again broke down, the controls reducing the areas were discontinued The crude threshold for form and area was taken as the last (least) area involved when the animals could discriminate the triangle from the circle

Reference to Table 4 will show that Goof broke down on control 36 when the areas were each 0.2165 square inches. Then, after responding correctly for 3 days on control 35 (0.433 square inches), he responded correctly on the areas of control 36. His discrimination next broke down at an area value of 0.0541 square inches. He was successfully retested for 3 days on areas of 0.1083 square inches, but he was unable to discriminate successfully the areas of 0.0541 square inches. Goof's "threshold value" in this crude estimation was thus taken as 0.1083 square inches for the triangle and the same area for the circle. Whether or not the monkey was responding positively to the positive stimulus or negatively to the negative stimulus was not ascertained at this time.

Bones broke down on control 33 with areas of 1.732 square inches but after responding correctly for three days on areas of 3.464 square inches he continued and did not break down again till the areas were reduced to 0.1083 square inches. Out of 10 trials he responded correctly 8 times. However, he was given 18 trials and since his last 14 were correct he was given a further reduction the following day. He broke down on these areas with an accuracy of 40% for 10 trials. On repeating this control after responding correctly for 3 days to the next larger stimuli he made an accuracy of 50%. Bones' "threshold value" then was considered to be 0.1083 square inches. This was the same as Goof's.

Squeak broke down with an accuracy of 80% on control 33 with areas of 1.732 square inches. It was thought that he could successfully discriminate smaller areas so the next day he was tested on further reduced areas. His accuracy was 70% for 10 trials. He responded correctly for 4 days on the areas of control 32 but, when retested on control 33, he again broke down, this time with an accuracy of 60%. Squeak's "threshold value" was thus taken as 3.464 square inches for each stimulus.

In the same manner Sis was tested till she broke down twice on the same control. Her "threshold value" as shown by Table 4 was 0.2165 square inches for each stimulus. These crude thresholds were 0.1083 square inches for Goof and Bones, 0.2165 for Sis, and 3.464 for Squeak.

All that can be shown by these results is that the animals were able to make detailed discriminations. A great deal cannot be said definitely concerning the monkey's visual acuity from these results

TABLE 4

RESULTS OF CONTROLS WHICH DECREASED THE AREAS OF THE TRIANGLE AND CIRCLE*

Control No	Areas in square inches	Goof†	Bones†	Squeak‡	Sis‡	Remarks
31	6 928	100	100	normal training area	normal training area	
32	3 464	100	100	100	not run	"Threshold" area for Squeak
33	1 732	93 (14 trials)	50§ 100//	80§ 60//	100	
34	0 866	100	100	70	79 (18 trials)	
35	0 433	100	100		100	
36	0 2165	70§ 100//	100		70§ 93// (14 trials)	"Threshold" area for Sis
37	0 1083	100	89 (18 trials)		60§ 60//	"Threshold" area for Goof and Bones
38	0 0541	60§ 60//	40§ 50//			

*Results expressed in percentage correct Percentages on basis of ten trials unless otherwise indicated

†Normal training area for Goof and Bones was 15 588 square inches

‡Normal training area for Squeak and Sis was 6 928 square inches

§First presentation

//Second presentation

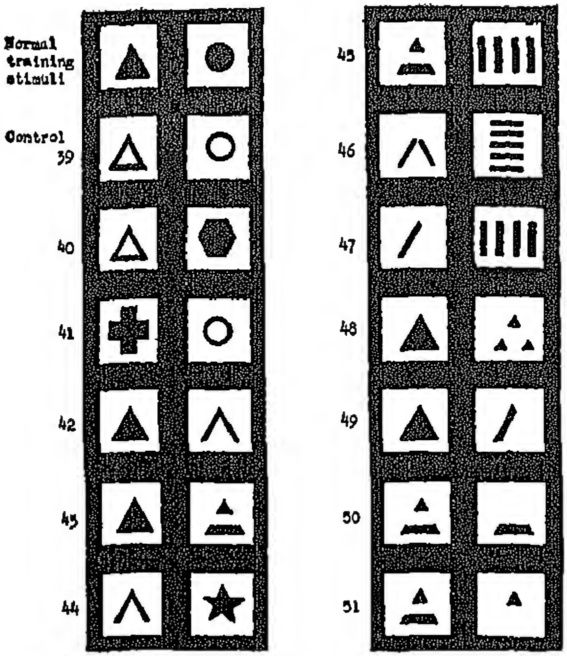

FIGURE 4
CONTROLS FOR RESPONSE TO PARTS OF TRIANGLE

because it was impossible to determine the distance from the stimuli at which the animals made their discriminations. The method of obtaining the threshold values was, of course, crude and the values are not highly comparable. Goof, Bones, and Sis discriminated much smaller areas than Squeak This may mean only that Squeak made his discriminations at a greater distance from the stimuli than the others. Goof's discriminations were made directly in front of and close to the stimuli He passed his head back and forth, his eyes at the level of the stimulus "as though carefully scrutinizing the stimuli in an effort to discriminate." Squeak's responses were rapid; he always rushed across the discrimination box and never hesitated in front of the door. His discriminations were probably made at quite a distance from the stimuli doors, which accounts for the fact that he could not discriminate accurately when the areas were reduced below 3.464 square inches.

5 *Results of Controls Determining Response to Parts of a Triangle.* This additional series of tests was given to only one animal, Goof. Since the responses of the other animals to controls 21 to 30 did not indicate an ability to continue with more difficult discriminations, they were not given these tests. After Goof had finished controls 21-29 (Figure 3) (both first test and retest) he was started on controls 39-51 (Figure 4) Table 5 gives the results for these controls These controls were designed to test the animal's ability to discriminate parts of the triangle. The black figures used in these controls were presented as usual in the center of the white square backgrounds In case the animal broke down on any one control he was given the usual three intervening days on the normal stimuli and on the fourth day the next control was presented In case the animal did not break down on a given control he was tested for one day on the normal training stimuli before being presented with the next control on the subsequent day.

Control 39 consisted of presenting the outlines of the normal training stimuli, i e., the outline of the triangle as a positive stimulus with the outline of the circle as a negative stimulus The outlines for both the triangle and the circle were 3/8 inch wide Goof's response to this control was 100% correct for 10 trials Control 40 presented the outline of the triangle as the positive stimulus with a regular hexagon (area 15 588 square inches) for the negative stimulus (In control 27 a hexagon had been used as a positive stimulus so in this

TABLE 5
RESULTS OF CONTROLS DETERMINING RESPONSE TO PARTS OF A TRIANGLE*

Control number	Goof
39	100
40	100
41	0
42	100
43	100
44	100
45	60
46	90
47	100
48	100
49	90
50	50
51	40

*Results expressed in percentage correct of 10 trials

case it could be used as a negative stimulus) He responded to this control with an accuracy of 100% for 10 trials which showed that he could definitely respond positively to the outline of the triangle. Control 41 substituted a black cross for the positive stimulus and used the outline of the circle for the negative stimulus The cross had been used as a negative stimulus in control 21 (58 days prior to its present use) so it was thought that it could serve adequately as a positive stimulus in this control. Goof responded negatively 10 trials in succession with an accuracy of zero There are two plausible explanations for such a response; either he had been conditioned to respond negatively to the cross to such an extent that he was still responding in such a manner or else he was responding positively as in control 25 (Figure 3) to a greater expanse of white background. It was definitely shown, however, that he did not respond negatively to the black outline of the circle And it could readily have been that in control 39 the response was only positive to the outline of the triangle without the black outline of the circle being effective in any way in the discrimination.

Control 42 presented the normal triangle as a positive stimulus with a negative stimulus which consisted of two lateral sides of the triangular outline. Each side was 6 inches long by ⅜ inch wide Naturally, as in controls 40 and 41, there was an outstanding difference in area between the positive and negative stimulus. (The difference in control 39 was not so great) Goof's accuracy here was 100%. That is, he responded positively in all 10 trials to the whole triangle The explanation of the response seems evident. The sides of the triangular outline did not contain a sufficient number of identical elements (identical with the whole triangle) to cause the response to break down It does not seem possible that the animal responded positively to the greater area alone of the whole triangle else he would have responded positively to the black cross in control 41.

Control 43 substituted an apex and a flat horizontal base, corresponding exactly to the dimensions and position of the regular normal triangle, for the negative stimulus The animal's responses were 100% correct The explanation for the behavior is the same as that for control 42 The negative stimulus consisting of apex and base did not contain a sufficient number of identical elements to cause the discrimination to break down.

In control 44 the two lateral sides of the triangle were used as a positive stimulus while a five-point star was used as the negative stimulus The area of the star was approximately 5.53 square inches while the area of the black sides was 4 313 square inches. Goof's accuracy was 100% for 10 trials. This control definitely showed that the animal could respond positively to the sides of the triangle Control 45 used the apex and base of the triangle for a positive stimulus and four vertical black stripes as the negative stimulus The stripes were each ¾ inch wide by 5 inches long and were placed 1½ inches apart. Their combined area was 15 square inches, while the combined area of the apex and base was 5.438 square inches (The triangular apex section was 1 inch high with a 1⅛-inch base, while the base section was 1 inch wide with a lower side length of 6 inches) Goof's response was only 60% correct He responded positively on the first five and on the ninth trials These results showed quite conclusively that Goof's discrimination in this instance was not based on any of the elements contained in the apex or base of the triangle They also show that he had obviously not been responding (entirely at least) to an area relation, i.e., positively to a larger area.

Control 46 used the two lateral sides of the triangle but of such a length that they did not form an apex The lines were each ⅜ inch wide by 4⅞ inches long. The negative stimulus consisted of 5 horizontal black stripes each ½ inch wide by 3⅞ inches long The stripes were ¾ inch apart. Goof's responses were 90% correct for 10 trials. This was an indication that in these situations Goof was responding to the elements contained in the sloping sides of the triangle rather than to those elements contained in the apex or base of the triangle. In the next control (47) only one of the sloping sides (width ⅜ inch and length 4⅞ inches) was used as a positive stimulus The vertical stripes of control 45 were the negative stimulus. Since the animal's accuracy for control 45 was only 60% he could not have become conditioned either positively or negatively to the vertical lines so they could still be used as a neutral negative stimulus Goof responded to the single sloping line for all 10 trials This was further proof that he was responding in these situations to a sloping side or section of the triangle and indicated that, when so required, the animal could respond to a very small part of the total distribution of light. In this case the discrimination was no doubt on a basis of shape, i e, the retinal distribution of light.

Control 48 used the normal triangle for the positive stimulus and three small triangles, each 1 inch in altitude, corresponding to the three points of the normal equilateral triangle The animal responded positively in all 10 trials. This control was used in an effort to determine whether or not the animal was responding largely on a basis of the elements composing the points of the triangle. Control 49 substituted the single sloping side of control 47 for the negative stimulus with the result that the animal's responses were 90% correct for 10 trials. He responded to the single sloping side only once. This control tended to show that, although the animal would respond to the sloping side of the triangle when presented along with some new, neutral negative stimulus, it would not do so when the sloping side is presented with the whole normal triangle.

Controls 50 and 51, which were responded to with accuracies of 50% and 40% respectively, indicated more conclusively that the animal's response in these abstract situations was not on a basis of either the apex of the triangle, or the base, nor on a combination of the elements composing them

Summary

a. Controls 39, 40, and 41 show that one animal, Goof, could respond positively to an outline of the normal triangle and that he probably could not respond negatively to an outline of the circle This latter point, however, was not thoroughly tested

b. Controls 43, 45, 50, and 51 show that in these situations, although the animal can no doubt respond to a differential distribution of light, he was not responding on a basis of the elements presented by the apex or the base of the triangle or by a combination of the two Control 48 shows, furthermore, that he was not responding to the three points of the triangle.

c. Controls 44, 46, and 47 show that the animal could discriminate on a basis of the lateral sides or only one side of the triangle

d Controls 42 and 49 show that the animal always responded to the entire triangle rather than to a given "abstract" portion such as the sides This indicates that, although the animal, when so required, can respond to the sides of the triangle or the shape aspect, he responds more readily on a basis of triangularity (form *per se*) or to the entire triangle, when presented with the triangle as a positive stimulus and the sides or side as a negative stimulus.

Conclusions

1. The four monkeys used in this experiment learned to discriminate between a black equilateral triangle, appearing in the center of a white square background, and a circle of the same area and brightness which also appeared in the center of a white square background. These discriminations were learned in 140 to 620 trials.

2. The backgrounds of the patterns were an essential part of the stimulating situation for three of the four animals in the first background control (combination 2, Figure 2). The backgrounds were essential in the second background control (combination 3, Figure 2) for only two of these three animals. After these first two controls, however, these three animals *were not influenced in any way whatever by alterations of the backgrounds*, or by changes in the positions of the forms appearing on the background. These three animals were apparently reacting at first to the form plus the background, although the relation of the two to form an adequate stimulus was so weak that subsequent controls caused the background factor to drop out as an essential stimulating element. The background was not an essential part of the stimulating situation in the discriminations of the fourth animal. The evidence indicates therefore that the animals were able to respond to a part of the total configuration presented. After the first two controls the animals were never again dependent on the background in their discriminations. Hunter's hypothesis is supported only by the results of the first two background controls.

3. By substituting various forms for those of the positive or negative stimulus it was found that all of the animals responded negatively to the negative form when the positive form was changed (three different controls) and that two of the animals responded positively to the positive form when the negative one was changed (four different controls). The other two animals responded positively in only one of these four latter controls. Therefore it was possible for the animals to discriminate correctly on the basis of one stimulus alone, either the positive or the negative. This type of response has been observed only once (Munn and Stiening working with the child) prior to this investigation. Because of these results and because of the readiness of all the animals to disregard the background factors of the stimuli, it seems evident that Gestalt characteristics of the stimuli are relatively unimportant determinants of behavior when tested under the present conditions.

4. One of the animals, Goof, demonstrated that he could respond to triangularity or form *per se.* The other three animals were unable to demonstrate such an ability. Their discriminations were on a basis of shape or the retinal distribution of light

Washburn (10) maintains that a response to form *per se* is due to the presence of "an abstract idea of triangularity." The contention of the present writer is that such a response may be evidence of the existence of a symbolic process controlling the reaction to triangularity or three-sidedness, without which a response to form *per se* would be impossible. But since the initial stimulus (triangularity or form *per se*) is external and always initiates the overt response, an explanation of the behavior is possible without postulating the functioning of a symbolic process I should therefore agree with the statement of Munn and Stiening that all that can be meant by a response to form *per se* "is that form is responded to regardless of its position in space and the background upon which it appears" (8, p 88).

5. Two of the animals were able to discriminate between an equilateral triangle and a circle of the same brightness both appearing on separate white square backgrounds when the areas of the forms were as small as 0.1083 square inches Another animal could discriminate with areas of 0.2165 square inches, while the fourth animal discriminated with areas only as small as 3.464 square inches Further evidence of the monkeys' ability to discriminate detailed visual pattern is shown by the responses of one animal in discriminating patterns which involved a small portion (one lateral side) of the triangle

6 The above experimental conclusions substantiate the prevailing morphological evidence that the Macacus rhesus monkey has a highly developed and efficient optical mechanism

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Clark University
Worcester, Massachusetts

LA DISCRIMINATION VISUELLE DES FORMES CHEZ LE MACAQUE RHÉSUS

(Résumé)

On a appris à quatre singes de discriminer entre un triangle équilatère noir (le stimulus positif) et un cercle noir de la même aire et de la même clarté, tous deux placés au centre de fonds blanc carrés Quand les animaux ont appris à discriminer avec une précision de 90 à 100%, on a employé divers contrôles qui ont changé la forme des fonds Les fonds ont été essentiels pour trois des animaux dans leurs premiers contrôles, mais non après Ils n'ont jamais été essentiels pour le quatrième animal Des contrôles qui ont substitué des figures nouvelles à une fois au stimulus positif et à une autre fois des figures nouvelles au stimulus negatif ont montré que tous deux des stimuli normaux de l'entraînement ont été actifs dans le contrôle du comportement puisque les animaux ont réagi négativement au stimulus négatif et positivement au stimulus positif Quand on a renverse le triangle et qu'on a employé un diamant comme stimulus negatif, seulement un animal a su discriminer correctement Cette réponse a été sur une base de triangularité ou de forme per se, tandis que celle des autres animaux a été basée sur la forme du triangle Les contrôles ont montré que trois des animaux ont su discriminer entre le triangle et le cercle quand les aires des deux stimuli ont été réduites à 0,1083 pouces carrés pour deux animaux et à 0,2165 pouces carres pour l'autre D'autres contrôles donnés à un animal ont montré qu'un côté latéral étroit du triangle a pu être discriminé de divers stimuli négatifs avec lesquels l'animal n'avait pas été entraîné Les resultats indiquent que les caractéristiques Gestalt des stimuli sont des déterminants relativement peu importants du comportement quand on les teste dans ces conditions-ci

NEET

DIE UNTERSCHEIDUNG VISUELLER GESTALTEN BEI MACACUS RHESUS AFFEN

(Referat)

Es wurden vier Maulaffen dazu dressiert, zwischen einem schwarzen, gleichseitigen Dreieck (dem positiven Reiz) und einem schwarzen Zirkel von der selben Grosse und Helligkeit zu unterscheiden. Beide Reize hatten viereckige, weisse Hintergrunde in deren Mitte sie als Vordergrund lagen Hatten die Tiere gelernt, mit einer Genauigkeit von 90 bis 100% zu unterscheiden, so wurden verscheidene Kontrollexperimente ausgefuhrt, in denen die Form der Hintergrunde geandert wurde Bei drei der Tiere erwiesen sich in den anfanglichen Kontrollversuchen die ursprunglichen Hintergrunde als zur richtigen Unterscheidung notwendig In den spateren Kontrollversuchen an diesen drei Tieren war dies nicht der Fall, und bei dem vierten Tier war es nie der Fall Kontrollversuche in denen neue Gestalten das einmal den positiven und ein andermal den negativen Reiz ersetzten haben erwiesen, dass die normalen, in der Dressierung verwendeten Reize beide in der Regulierung des Verhaltens wirksam waren, da die Tiere auf den negativen Reiz negativ und auf den positiven Reiz positiv reagierten Wendete man den Dreieck um, und gebrauchte man einen Rhombus [diamond] als negativen Reiz, so war bloss ein einziges Tier im Stande, richtig zu unterscheiden Die Reaktion dieses Tieres fand auf Basis der Dreieckigkeit oder der Form *per se* statt, wahrend die Reaktion der anderen Tiere auf Basis der Form des Dreiecks stattfand Kontrollversuche haben erwiesen, dass drei der Tiere auch dann im Stand waren, zwischen Dreieck und Zirkel zu unterscheiden, wenn der Flacheninhalt der zwei Reize fur zwei Tiere bis auf 0 1083 und bei dem anderen bis auf 0 2165 sq inches vermindert worden war Weitere Kontrollversuche die an einem Tier ausgefuhrt wurden haben erwiesen, dass eine schmale Seite des Dreiecks von verschiedenen negativen Reizen, mit denen das Tier nicht dressiert worden war, unterschieden werden konnte Die Befunde weisen darauf hin, dass Gestalteigenschaften der Reize relativ unbedeutend als Einwirkungen auf das Verhalten [behavior] sind, wenn man letzteres unter den Bedingungen der gegenwartigen Untersuchung pruft

NEET

A STUDY OF THE PERSONALITY ADJUSTMENTS OF ONLY AND INTERMEDIATE CHILDREN*

From the Department of Psychology of the University of Oregon

ALBERT A CAMPBELL

Theory has traditionally held that the child raised without brothers or sisters is especially predisposed to the development of personality difficulties and unsatisfactory social adjustments Research has, in general, not borne out this position, although some studies indicate that it is perhaps not entirely without foundation.[1]

This research has, however, been characterized by two important weaknesses. In the first place, the experimental studies have been inclined to ignore the differences which surely existed in the intellectual and economic backgrounds of their subjects' homes. The presence of these differences has been pointed out by Guilford and Worcester (8) and Thurstone and Jenkins (11), and their importance is discussed by Hsiao (9) as follows "The social and economic status of the family is undoubtedly an important factor in determining some of the traits of the children. It is easily conceivable that if the material is heterogeneous in its social composition, it is likely to create spurious differences among birth orders if averages are taken without controlling the social and economic status of the family." Inasmuch as the previous studies almost without exception report significant differences in the average intelligence ratings of the groups they were comparing, and since we know children's and parents' intelligence to be significantly related, the possibility of error in the conclusions reached by these researches is considerable.

A second weakness of these recent studies has been in the fact that they nearly all deal with children of less than high-school age. It is true, of course, that various authors have suggested that the only child, as the result of his unusual environment, is apt to experience particular difficulty in severing home ties and standing on

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[1]The writer has prepared a review of this literature for publication elsewhere (5)

his own feet, an ordeal which would conceivably bring to light definite personality difficulties But why should these peculiarities be expected to appear in kindergarten and grammar-school children who have hardly taken the first step toward independence of parental supervision? Not only is their environment comparatively free of trying circumstances but also their reaction tendencies are still in a formative state and not nearly as well differentiated as they will become later It seems evident that if family position has any influence upon personality adjustment this influence will be best observed at an age more mature than kindergarten level.

Two studies have been done on higher age levels, one at Colgate University by Stuart (10) and the other at the University of Ohio by Fenton (7). Neither of these researches revealed any significant effect of birth order on personality. However, in the former case no consideration was given the relative intelligence of the subjects tested and in the latter study the groups compared were shown definitely to be not comparable on the basis of this variable Appreciable and consistent differences in the social-economic backgrounds of the subjects might well be expected

The possibility of sex differences in the reaction to the family situation has been largely neglected

Procedure

In this study the influence of the circumstances attendant to being raised with or without siblings was investigated by a comparative study of two groups of students at the University of Oregon. One group was composed of students who had been raised as only children, their adjustments being in no way affected by the presence of brothers or sisters The experimental group was comprised of a like number of students, comparable in many ways, but who had been exposed to continual fraternal or sororal relationships, students who had been brought up as intermediate children with at least one older and one younger sibling, who had never been the oldest child in the family and in most cases were the youngest for only a few years None of the subjects was living at home at the time of the experiment.

Two hundred students, 100 men and 100 women, all undergraduates, acted as subjects Half of this number, 50 of each sex, were only children. They represented all four college classes and as nearly as possible a normal cross-section of the campus population

None of this control group had been brought up with other children, either related or otherwise. The experimental group of 100 students raised as intermediate children was paired with the group of only children for intelligence, sex, and school year. The intelligence pairing was done both on the basis of the student's score on the American Council on Education intelligence examination given at entrance to the university and on his high-school scholastic record. The pairing correlations (computed from a composite of these two scores rather than from each individually) were $r = .97$ for the men and $r = .93$ for the women. There were no discrepancies in the pairing for sex and school year.

The pairing for intelligence was done for two reasons. First, by carefully controlling the intelligence variable of the subjects some of the varying socio-economic status of their families could be eliminated since socio-economic status varies with intellectual ability of parents, which, in turn, varies directly with the intelligence of their children. This control was obviously not very exact. An effort was made to control this variable more accurately but it proved impractical with the facilities which were available. Secondly, the *intelligence factor was controlled to make feasible a valid comparison* of the college scholastic records of the two groups. The fallacy of comparing the grade records of students not paired for intelligence is obvious.

The pairing for college class was done in light of the possible influence of the college environment on personality adjustments and the likelihood of finding distinct differences between freshmen and seniors. The pairing for sex was done as an added control.

Data falling into three distinct categories were obtained. First and most important were the data concerning the personality traits on which only and intermediate children would supposedly differ. These data were gained through the use of the Bernreuter Personality Inventory (2), designed to give ratings for neuroticism, self-sufficiency, introversion-extraversion, and dominance-submission, and Hulsey Cason's Annoyance Test (6), giving a score for general irritability. In addition to this, information was gathered as to the comparative college scholastic records of the two groups, compiled from the University records. The third class of data consisted of physical ratings of the subjects, obtained from the University health service.

TABLE 1

MEAN SCORES OF ONLY AND INTERMEDIATE CHILDREN ON PERSONALITY AND SCHOLASTIC RATINGS

(The notation + indicates the position of the only-child group as compared to the intermediate group)

	Women				Men			
	Diff in mean scores	σ_d	D/σ_d	Chances in 100	Diff in mean scores	σ_d	D/σ_d	Chances in 100
Neuroticism	+25 2	16 40	1 54	93	+5 6	15 67	327	64
Self-sufficiency	— 9 6	10 25	937	83	+5 4	10.85	.498	69
Introversion	+15 1	10 05	1.50	93	+ 7	9 68	072	52
Dominance	— 8.2	12 12	677	76	+4 2	11 91	353	64
Annoyance	+ 18	734	245	60	+ 28	786	356	64
Grade points per hour carried	— 01	333	030	52	— .12	499	240	60

Results

A comparison of the mean scores of the paired groups of subjects, both men and women, on the five personality ratings and a consideration of their respective college scholastic records were made (cf. Table 1). The striking feature presented by this table is the close similarity of the means of the two groups of men as compared to the rather significant disparities in the mean scores of the women. The greatest difference in the mean scores of the only and intermediate men was not quite one half the sigma of that difference while, on the other hand, in the women's mean scores differences appeared ranging up to more than one and a half times the sigma of the difference.

From this table it will be noted that on ratings of neuroticism, self-sufficiency, dominance, and irritability, the male students raised without brothers or sisters made slightly higher scores than did the students raised as intermediate children. They made practically the same average on an introversion-extraversion scale, and the intermediate child group achieved a slightly better grade point average. Women raised as only children made significantly higher scores than did intermediate women on scales measuring neuroticism and introversion, and made definitely lower scores on ratings of self-sufficiency and dominance. They made a slightly higher mean score on a test of irritability than did the paired group, and a practically identical grade point average.

In Table 2 are presented the variations of the scores of the four groups tested. In all four parts of the Bernreuter Inventory the sigmas of the scores of the students, both men and women, raised as only children were wider than those of the intermediate children, the chances of these differences in sigmas being due to mere chance ranging from 58 to 42 down to 96 to 4. These differences in sigmas were a little more prominent among the men. The men raised as only children were consistently more variable in their scores on the Bernreuter scale, the difference over the sigma of the difference ranging from 1.22 to 7.11. They also seemed to make slightly more variable scores on the Cason test but their grade point averages were less scattered than those of the intermediate group. The women raised as only children had a much wider sigma than the intermediate group in their scores on the dominance rating, were somewhat more variable on the neuroticism and self-sufficiency scales and a little more variable in the introversion rating. On the

TABLE 2

VARIABILITY OF SCORES OF ONLY AND INTERMEDIATE CHILDREN ON PERSONALITY AND SCHOLASTIC RATINGS
(The notation + indicates greater variability among the only-child group)

	Women				Men			
	Diff in sigmas	σ_d	D/σ_d	Chances in 100	Diff in sigmas	σ_d	D/σ_d	Chances in 100
Neuroticism	+12 09	11 61	1 04	85	+15 55	11.08	1.22	88
Self-sufficiency	+ 3 61	7 25	498	69	+ 6 65	7 67	867	80
Introversion	+ 1 33	7 14	186	58	+ 6 36	6 85	928	83
Dominance	+14.63	8 56	1 71	96	+ 5 99	8 42	711	76
Annoyance	— 06	52	115	54	+ 10	556	180	58
Grade points per hour carried	— 110	105	1 05	85	— 073	112	652	74

Annoyance Test the two groups were practically the same, but the only women were rather more consistent in their grade point averages than the intermediate women

The scores of the four groups on the six variables were separately intercorrelated[2] to show any possible further differences. None of the groups was distinguished by any unusual relationships not present in the scores of the others.

A comparison of the physical ratings of the 200 subjects showed no differences of any magnitude to exist in the ratings of the students raised as only children and those raised with siblings.

Discussion

The evidence presented in the experimental findings of this research indicates that the family situation attendant to the presence or absence of brothers or sisters in a child's early environment has a significant and lingering influence upon his personality adjustment. It is also *inferable from the data that, in respect to the development* of normal behavior tendencies, it is somewhat more advantageous for a girl to be raised in a family with siblings than it is for a boy This same conclusion was reached recently by a German worker, Busemann (4), as the result of his extensive study of some 400 children ranging in age from 10 to 17 years. He likewise found *definite discrepancies in the personality traits of only boys and only* girls and concluded that the "Geschwisterschaft" is more beneficial to the girl than the boy

It is conceivable that the tendency of both only-child groups to seek the extremes of the personality ratings, which has also been noted by Blonski (3) and Bender (1), is due to an inability of the parents of an only child to refrain from over-indulging or overprotecting their offspring. They do not learn, as do more experienced parents, to strike a middle course in the raising of their child.

While this tendency toward an unbalanced training probably appears just as consistently in the case of boys as of girls, its influence is apparently somewhat less effective in the case of the former Boys are traditionally allowed more freedom than their sisters, and are exposed to a much wider range of extra-parental influences Parents are frequently inclined to allow a rather free rein to their

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[2]The complete data obtained in this study are available in the library of the University of Oregon

sons and it is possible that this leniency should be even more pronounced in the case of an only son. It is noteworthy that the greatest differences appearing between mean scores of the only and intermediate men students tested in this study were the somewhat higher scores made by the former on the dominance and self-sufficiency ratings.

Such a situation is not the case among the daughters of the house. They are diligently protected from girlhood on. Again it would not be surprising if the parents of an only daughter were unusually zealous in coddling and protecting their child. It is interesting that the women raised as only children who acted as subjects in this experiment made higher mean scores on the neuroticism and introversion scales and lower mean scores on dominance and self-sufficiency ratings than did the paired group who had been raised with brothers and sisters.

From the intercorrelations of the four divisions of the Bernreuter Inventory it is evident that this test does not give as broad an indication of personality as one might be led to believe. In the 200 tests given in this study the neurotic scale correlated with the introversion scale $r = .96 \pm .004$, while the correlation of the neurotic scale with the dominance rating was $r = -.84 \pm .014$. The large common element present in the variables measured by these three tests makes it scarcely worth while to use more than one of the ratings.

Summary

A study has been done to determine the effect of certain family relationships upon personality development and some other measures. Two hundred college students acted as subjects, 100 of each sex. The subjects were paired for family position, intelligence test score, college class, and sex. It has been found that:

1. Only children appear to be somewhat predisposed by their early environment to the development of unusual personality traits. The boys raised as only children made higher scores on tests of neuroticism, self-sufficiency, and dominance than did the boys raised as intermediate children. The girls raised as only children made higher scores than did girls raised with brothers and sisters on ratings of neuroticism and introversion and lower scores on tests of dominance and self-sufficiency. Both only-child groups made significantly more variable scores than did the group with which they were paired.

2. This tendency toward unusual personality adjustments is more pronounced in women than in men.

3 No differentiation may be drawn between only and intermediate children in regard to either physique or scholarship

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Stanford University
California

LES AJUSTEMENTS DE PERSONNALITÉ CHEZ LES ENFANTS UNIQUES ET LES ENFANTS INTERMÉDIAIRES

(Résumé)

Les ajustements de personnalite chez cent enfants uniques et le même nombre d'enfants élevés avec au moins un frère aîné ou une soeur aînée un frère plus jeune ou une soeur plus jeune ont été mesurés par l'Inventaire de Personnalité de Bernreuter et le Test d'Ennui de Hulsey Cason Les deux cents sujets ont tous été des étudiants universitaires, dont cent de chaque sexe, et les groupes des enfants uniques et des enfants intermediaires ont été mis en paires sur la base du résultat du test d'intelligence, du sexe, et de l'annee universitaire

On a trouvé que les circonstances qui se presentent quand on est élevé sans freres ni soeurs prédisposent un peu un enfant au développement de quelques ajustements de personnalité peu communs Cette influence est

plus marquée chez les filles que chez les garçons On n'a trouvé aucune base pour une généralisation à l'égard d'une infériorité physique ou scolaire chez l'enfant unique

CAMPBELL

PERSÖNLICHKEITSANPASSUNGEN "EINZIGER" UND ZWISCHENKINDER

(Referat)

Die Persönlichkeitsanpassungen [personality adjustments] von 100 einzigen [only] Kindern, und von einer gleichen Anzahl von Kindern die mit wenigstens einem älteren und einem jüngeren Geschwisterkind (Bruder oder Schwester) grossgezogen worden waren, wurden mit dem Bernreuter'schen Persönlichkeitsinventarium [Bernreuter Personality Inventory] und dem Belastigungstest von Hulsey Cason [Hulsey Cason's Annoyance Test] gemessen Ein hundert der Versuchspersonen waren Studenten und die anderen 100 Studentinnen Es wurde immer ein einziges Kind mit einem Zwischenkind gepaart, in Bezug auf den an Intelligenzprüfungen erzielen Rang, auf Geschlecht, und auf den Dauer des Studententums.

Die Befunde haben erwiesen, dass die im Erwachsen ohne Bruder oder Schwester mit einbegriffenen Umstande ein Kind einigermassen dazu geneigt machen, etwas ungewohnliche Persönlichkeitseigenschaften zu entwickeln Diese Einwurkung ist bei Mädchen ausgeprägter, als bei Knaben Man hat keine Basis entdeckt für Verallgemeinerungen über physische oder scholastische Minderwertigkeit bei einzigen Kindern

CAMPBELL

SHORT ARTICLES AND NOTES

THE MEASUREMENT OF HANDEDNESS

C. E Lauterbach

Various methods have been evolved for the measurement of handedness Among these are anatomical measurements, tests of eyedness, strength of grip, the shot-tube test, the steadiness test, the peg board, star tracing, and others But handedness cannot be satisfactorily measured by a single performance What is obviously needed is a cross-section of an individual's manual activities

This conclusion, after several years of experimentation, led to the development of the "Habit Scale for Handedness," which is given at the end of this article The scale contains 50 items chosen to cover a wide field of activities for both children and adults Each activity is scored on the basis of 10 If it is habitually performed with the right hand, a score of 10 is entered for the right hand, if it is habitually performed with the left hand, a score of 10 is entered for the left hand If an activity is performed with equal efficiency with either hand, each hand is scored 5. If an activity is performed with both hands, but slightly more efficiently with one than with the other, an estimated division of the score is made, as (6) (4), (7) (3), (2) (8), (4) (6), etc The scores for each hand are then added and totaled. The score of the right hand is divided by the total score for both hands, yielding the percentage of right-handedness in the total performance Reference to the illustration of the scale will make the procedure clear.

The percentage of right-handedness as measured by the scale is multiplied by 100 to eliminate the decimal point and the result is called the "Index of Handedness." A range of indexes from 0 to 100 is thus secured Where left-handedness is absolutely dominant, the I.H will be 0, where right-handedness is absolutely dominant, the I H. will be 100 True ambidexterity will be indicated by an I H of 50 An I H. of 30 indicates left-handedness but not dominance, an I.H of 70 indicates right-handedness but not dominance, and so on.

The scale was administered to 1061 individuals ranging in age from less than a year to 70 years and about equally divided as to sex First, approximately five hundred individuals were measured who said they were left-handed; then an approximately equal number were measured who said they were right-handed The distribution of indexes is shown in Table 1 Forty-five and one-tenth per cent of those measured proved to have indexes under 50, 54.9%, 50 or above.

The "curve of handedness" seems to be U-shaped but is considerably skewed toward right-handedness (see Figure 1) The mode for the right-handed group is in the last interval, 95-100 For the left-handed group one would expect the mode to fall in the first interval, 0-4 The fact is that

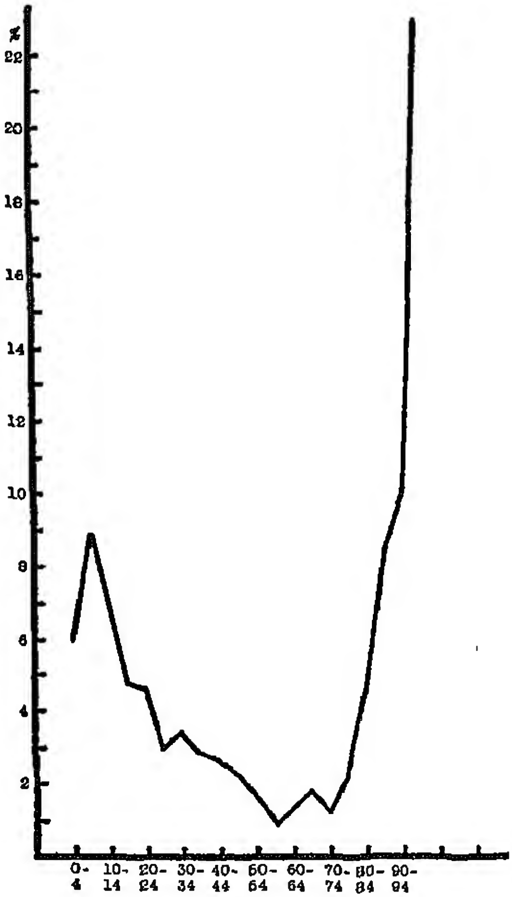

FIGURE 1

Showing a Distribution of the Index of Handedness in a Population of 1061 Individuals

The horizontal axis represents the index, the vertical axis, the frequency of occurrence in terms of percentage.

TABLE 1

DISTRIBUTION OF THE INDEX OF HANDEDNESS GROUPED ACCORDING TO (I) SUCCESSFUL TRANSFER IN PENMANSHIP FROM LEFT TO RIGHT, (II) UNSUCCESSFUL ATTEMPT AT TRANSFER, (III) NO ATTEMPT AT TRANSFER, (IV) NATURAL RIGHT-HAND WRITERS

	Right-hand writers		Left-hand writers			
Index	IV	I	II	III	Total	%
0- 4			29	34	63	5.9
5- 9		14	26	54	94	8.9
10-14		24	24	25	73	6.9
15-19		17	23	11	51	4.8
20-24		19	19	11	49	4.6
25-29		12	10	10	32	3.0
30-34		15	11	10	36	3.4
35-39	1	16	6	7	30	2.8
40-44	1	16	7	4	28	2.6
45-49		15	3	5	23	2.2
50-54	2	11	2	2	17	1.6
55-59	1	8	1		10	.9
60-64	8	5		1	14	1.3
65-69	7	9	2	1	19	1.8
70-74	9	3	1		13	1.2
75-79	17	4		2	23	2.2
80-84	42	4		2	48	4.5
85-89	83	4		2	89	8.4
90-94	101	5			106	10.0
95-100	240	2	1		243	22.9
Total	512	203	165	181	1061	99.9

it falls in the second interval, 5-9. The curve is much steeper from 50 to 100 than it is from 50 to 0. The data suggest the influence of environment and training. The left-handed person finds himself in a right-handed world and so, willy-nilly, he tends to become right-handed. In other words, the environment imposes a certain amount of transfer.

The U-shape of the curve indicates the lack of complete manual dominance. Strictly speaking, only that individual who has lost the use of his left arm is dominantly right-handed, and vice versa. In between the extremes are varying degrees of left- and right-handedness, with the general tendency away from ambidexterity.

The downward fluctuation of the curve in the interval 0-4 may have some bearing on the heredity of handedness and also on the problem of transfer in penmanship. It may be noted that in the group with indexes of 0-4 no successful transfers were found.

The validity of the scale depends upon its content. If the 50 items listed may be considered a representative cross-section of an individual's manual activities, the scale should yield a fairly valid measure of dextrality Each item was carefully selected The "Manual of Instructions" contains definitions of handedness where confusion might arise In the case of bimanual activities exception may be taken to some of the definitions offered In the case of shoveling, for example, it may become necessary eventually to define handedness in terms of the type of lever (i.e, first or third class) represented in the activity rather than by the position of the hand on the handle of the tool In any event, the scale represents a standardized instrument yielding an objective unit of measurement.

The reliability of the scale was tested in the following manner A group of graduate students measured the handedness of 563 individuals, approximately half of them left-handed Six weeks after the first examination, 328 of the original group were remeasured by another group of graduate students and a correlation ratio (eta) was calculated between the indexes obtained in the first examination and those obtained in the second. This correlation ratio proved to be 978 ± 036. A high degree of reliability is thus indicated despite the fact that when the score for any one item has to be divided an estimate is involved

The mean time required for the administration of the scale as indicated by 412 cases is 10.6 minutes

Several uses for the scale suggest themselves:

1 It should be useful in the diagnosis of handedness when the question of transfer in penmanship is involved.

2. It should help to determine the relationship between handedness and speech disorders

3. It should make possible the determination of the degree of handedness transmitted by parents to offspring, whether by heredity or training

4 It should aid in the accurate determination of the various degrees of handedness in the population as a whole.

Examiner's Manual

To use the scale determine whether the activities listed are performed with the right or the left hand by saying, "Show me how you write" or "Show me how you throw a ball" etc. With children it is helpful to equip oneself with several of the objects and tools mentioned in the scale Small hoes, rakes, shovels, etc., such as children play with, permit actual demonstration when doubt arises. Adults can usually demonstrate without equipment or can rely upon their memories

If an activity is habitually right-handed, assign it a score of 10 in the column headed "R"; if it is habitually left-handed, a score of 10 in the column headed "L". If an activity is performed habitually without preference for either hand, assign a score of 5 in each column If an activity

HABIT SCALE FOR HANDEDNESS

FORM 101 C E L 6-1-1931 No

(1) Name First Last
(2) Next birthday Month Day Year
(3) How old will testee be then?
(4) Date of test Month Day Year
(5) City State
(6) Street No
(7) Vocation of father
(8) Vocation of testee

(9) Index
(10) Sex
(11) Age
(12) I Q.
(13) Grade
(14) Writing
(15) Transfer?
(16) Sp'ch def?

SHOW ME HOW YOU		R	L
A	1. Point (with finger)	( )	( )
	2. Hold rattle (spoon)	( )	( )
	3 Pick up (blocks, pins)	( )	( )
	4 Throw (a ball)	( )	( )
	5 Shake hands	( )	( )
	6. Open a door	( )	( )
	7 Brush your teeth	( )	( )
	8 Play marbles	( )	( )
	9 Roll a hoop	( )	( )
	10. Draw (a picture)	( )	( )
B	11 Write (with pen)	( )	( )
	12. Eat (soup)	( )	( )
	13 Wind a clock (watch)	( )	( )
	14 Deal cards	( )	( )
	15 Swat a fly	( )	( )
	16 Strike a match	( )	( )
	17. Hold a glass (cup)	( )	( )
	18 Cut with shears	( )	( )
	19 File finger nails	( )	( )
	20 Stir (coffee, tea)	( )	( )
C.	21 Shave	( )	( )
	22 Shovel (coal, sand)	( )	( )
	23 Hoe (in the garden)	( )	( )
	24 Rake (leaves)	( )	( )
	25 Fork (hay)	( )	( )
	26 Saw (a board)	( )	( )
	27 Hammer	( )	( )
	28 Chop (with axe)	( )	( )
	29 Mow (with sickle)	( )	( )
	30 Whittle	( )	( )
D	31 Cut bread	( )	( )
	32 Beat eggs (with spoon)	( )	( )
	33 Sift flour	( )	( )
	34 Pour coffee (tea)	( )	( )
	35. Wash dishes	( )	( )
	36 Peel (potatoes, apples)	( )	( )
	37 Iron (clothes)	( )	( )
	38 Sweep (with broom)	( )	( )
	39 Sew	( )	( )
	40 Thread a needle	( )	( )
E	41 Throw javelin (spear)	( )	( )
	42 Bat a ball	( )	( )
	43 Play tennis	( )	( )
	44 Play golf	( )	( )
	45 Shoot billiards (pool)	( )	( )
	46 Pitch horse shoes	( )	( )
	47 Shoot (gun, pistol)	( )	( )
	48 Cast with rod	( )	( )
	49 Play ukulele (violin)	( )	( )
	50 Play trombone (cornet)	( )	( )

		R	L
Total	A	( )	( )
	B	( )	( )
	C	( )	( )
	D	( )	( )
	E	( )	( )

Summation

TS (Total Score) $= \Sigma R + \Sigma L =$

$$\text{INDEX} = \frac{\Sigma R}{TS} =$$

Examiner

is performed with both hands, but more frequently with one than with the other, estimate the approximate score value for each hand, i.e. (6) (4), (8) (2), etc If the activity has never come within the experience of the testee, do not score it at all

In activities involving only one hand, handedness is easily determined. In writing, sewing, throwing, whittling, hammering, etc the activity is right-handed if the tool is held in the right hand.

In the case of bimanual activities the matter is not always so clear. For the sake of uniformity handedness will be tentatively defined as follows for the activities listed in the scale Shoveling, hoeing, raking, forking, sweeping, chopping, batting and playing golf are right-handed when the right hand is below the left on the handle of the tool (in chopping, for example, when the right hand is toward the head of the ax) Shooting billiards is right-handed when the right arm propels the cue, the fingers of the left hand serving as the bridge In the case of string instruments, the performance is right-handed when the bow or pick is held in the right hand In the case of wind instruments, the performance is right-handed when the slide or the valves are manipulated with the right hand

Filing the nails seems to be a special case If the file is actively manipulated by the right hand while the left hand is held motionless, and if the nails of the right hand are moved back and forth across the file while it is held stationary by the left hand, the performance is right-handed Left-handedness is just the reverse and ambidexterity requires that the file be actively manipulated by each hand

Note that the point involved in the scale is one of *habit* It is not a question whether the testee *can* write with either hand but whether he *actually does* When put to the test any activity can be performed with either hand but of course not with equal efficiency Many cases of true ambidexterity are on record and intermediate cases between complete dominance and ambidexterity are numerous In fact, the rare case is one of complete dominance To illustrate, many left-handed folks have learned to write with the right hand, some habitually shake hands with the right hand, some use shears with the right hand, etc Some right-handed folks deal cards with the left hand, and some few even write with the left hand

An inventory of activities as suggested by the Habit Scale yields a fairly reliable sample of an individual's manual habits The Handedness Index is an expression of the percentage of right-handedness It is found by dividing the score of the right hand by the total score yielded by the scale The following classification of Indexes is suggested 0-19, dominant left-handedness, 20-39, left-handedness but not dominant, 40-59, ambidexterity, 60-79, right-handedness but not dominant, 80-100, dominant right-handedness

West Virginia Wesleyan College
Buckhannon, West Virginia

THE MEASUREMENT OF AGGRESSIVE BEHAVIOR IN LABORATORY RATS

FRANK C DAVIS

It is the purpose of this note (1) to describe changes in tendencies toward "aggressiveness" which occur when the rat's environment is altered in certain ways and (2) to suggest a scale of aggressiveness for rats.

EXPERIMENT

In the course of some observations directed toward another end, I noticed that white rats which were living alone, each in his own cage, frequently attacked strange rats which were put into their cages[1] The attacks ranged from mere "pushing" movements to savage attacks which would probably have ended in the death of the intruder except for my intervention The attempt was then made to study systematically some of the factors producing this type of aggressive behavior, and to get light on the genesis of the individual differences noted with respect to degree of aggressiveness During the course of this study various criteria of aggressiveness were listed, and these form the basis of the rating-scale technique to be described below

The "subjects" were eight adult male rats When the writer received them they were in two cages, four in each cage The rats were left for a period of one week in these groupings, in order that their behavior might be studied under the conditions to which they had become accustomed, to find out, for example, whether or not certain ones of them were habitually more vicious or aggressive than the rest They were observed in the forenoon and afternoon each day, and on three occasions in the evening also During the course of these observations no fights, no acts of aggression on the part of one rat toward another rat, were noted The rats were now separated, each of four of the males of one cage being put alone in a cage isolated from the cages occupied by his former mates The other four males were kept apart in their original cage, and were used throughout the subsequent observations as "stimulus" or "stranger" rats After two days had elapsed, to allow the rats which had been moved to become accustomed to their new living conditions, the following experimental situations were introduced

(*a*) At different times in the day, both at feeding and at non-feeding times, a "stranger" rat was put into the cage of an "at home" rat, and the behavior of the two rats was noted *All rats fed once every 24 hours*

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[1]These observations were made in the Animal Laboratory of the Psychology Department of the University of California I wish to thank Professor E C Tolman for putting the animals at my disposal and affording me opportunity to make the observations herein reported

(*b*) At different times in the day, both at feeding and at non-feeding times, *two* "stranger" rats were introduced into the cage with an "at home" rat, and the behavior of the three rats noted *All rats fed once every 24 hours as above*

(*c*) At the time of feeding, a "stranger" rat was put into the cage of an "at home" rat, and observations made of the resulting behavior *All the rats, both "at home" and "stranger," fed only once every 48 hours*

(*d*) At the time of feeding, two "stranger" rats were put into the cage with an "at home" rat *As in (c), all rats fed only once every 48 hours*

In the (*a*) part of the investigation, observations were made on five successive days, in the (*b*) part, on three successive days, in (*c*), observations were made at the time of feeding on six occasions, in (*d*), at the time of feeding on two occasions Each series of observations followed on the next day after the conclusion of the preceding series.

Results Under situation (*a*) none of the isolated "at home" rats manifested aggressive behavior toward the "stranger" rat Observations were made for a period of three-quarters of an hour each, on two days once in the morning and once in the afternoon (before feeding time), and on three days in the afternoon (at feeding time) and in the evening At all these times the "at home" rats gave some slight attention to the "stranger," as evidenced by nosing and smelling. The "stranger" rats went through the usual explorations of a rat in a new environment, ate food along with the "at home" rat on those occasions when observations were made at feeding time, and then made themselves at home. The result was the same with (*b*), when two "stranger" rats were simultaneously introduced into the cage of an "at home" rat This was contrary to my expectations, since my earlier incidental observations had led me to expect aggression on the part of the "at home" rats toward the "stranger" rats under such conditions Two explanations of these negative results suggested themselves (1) the preliminary study of these rats in their accustomed environments had indicated that they might be an unusually unaggressive, easy-going lot, inasmuch as a week's observations had failed to disclose a single act of aggression on the part of any of them toward any other, (2) the two-day period of isolation might not have been long enough for the "at home" rats to develop a "sense of property rights" or whatever is back of the antagonism frequently manifested by a rat, living alone, toward an intruder

But the behavior picture changed radically under the conditions described above under (*c*) and (*d*). The "at home" rats were the same four males used as such in the part of the investigation described under (*a*) and (*b*) As noted above, both "at home" rats and "stranger" rats were now fed only once in every 48 hours Observations were made only at the time of feeding In the case of each of the four "at home" rats, in the (*c*) part of the experiment, the "at home" rat took on an aggressive attitude toward the

"stranger" rat immediately *after* the food was all eaten They were both hungry, of course, and in the case of the "stranger" this operated to inhibit the usual exploring of his unfamiliar surroundings But, while the "at home" rat suffered the "stranger" to eat beside him as long as the food lasted, as soon as it was all gone he made known his resentment at the other's presence by advancing against him and crowding him into a corner or against the side of the cage This behavior was strikingly dissimilar to the indifference shown by these same "at home" rats to the presence of the "stranger" rats when they were being fed every day. The aggressiveness, in the case of every rat at every observation period, lasted as long as the observer watched—from three-quarters of an hour to an hour and 15 minutes on different occasions However, even under these conditions, the aggressive behavior noted did not, with these animals, terminate in a life-and-death combat (The "at home" rats in the earlier incidental observations previously noted were larger, more powerful rats than the ones here used, and they often—even when not hungry—attacked intruders viciously, so viciously that I had to be on the alert to separate them in time to prevent a fatality This vicious purposiveness was lacking in the aggressiveness displayed by the "at home" rats in the present experiment In no cases did they actually bite the stranger, but simply kept pushing and crowding him, refusing to tolerate his presence, as long as the observation period lasted) In the (*d*) part of this experiment, when two "stranger" rats were put in the cage along with the container of food, the "at home" rats reacted similarly They gave all their attention to eating as long as the food lasted, and then began overtly to "resent" the presence of the two strangers *Always, in both (c) and (d), it was the "at home" rat who took the aggressive attitude, never one of the intruders.* The "at home" rats seemed to get even more excited over the presence of two strangers than they had at the intrusion of one Typically, the "at home" rat sidled toward first one, then the other of the two unwelcome guests, crowding them against the side of the cage, "puffing" and "snorting," hair bristling As in (*c*), however, the "at home" rats did not come to the point of actually attacking the strangers, but merely kept continually after them, not allowing them a moment's peace, constantly crowding them and pushing them against the side of the cage

Summary of experimental results White rats which had shown no tendencies toward aggressiveness under accustomed conditions of group living failed to develop aggressive tendencies toward intruders when isolated, one to a cage, as long as they were fed once a day as usual With the lengthening of the interval between feedings to forty-eight hours, however, definite and persistent aggressive behavior developed on the part of every "at home" rat—after eating—toward "stranger" rats put into their cages with their food

A Scale for Aggressiveness

Aggressiveness in rats can be measured in terms of (*a*) intensity of aggressive behavior, or (*b*) its duration Of these, "intensity" is the more unambiguous measure if its "degree" be agreed upon and specified in a rating scale "Duration" may be ambiguous if used alone, since a short duration might mean either very little aggressiveness or an attack so vicious that the object of the aggression is dispatched very promptly I shall list *certain criteria of aggressiveness—behaviors manifested by white rats under* different stimulating conditions—and shall suggest a seven-point scale of intensities to which these criteria readily lend themselves The proposed criteria are as follows

(*a*) Persistent crowding, pushing movements of one rat toward another

(*b*) "Puffing" and "snorting" sounds on the part of the aggressor

(*c*) Erection of the hair.

(*d*) Biting movements, or "threats," by the aggressor, without actual bites being administered

(*e*) Actual biting on the part of the aggressor.

(*f*) A form of restlessness on the part of one rat shown only under certain conditions when another rat (or rats) is present in the first rat's cage, and characterized by *occasional "sidling" movements in the direction of the* other rat (This is sometimes observed when a strange rat is introduced into a cage at the same time that food is put in, and differs from (*a*), above, in that the aggressive movements are intermittent rather than continuous, the "at home" rat's time being divided between eating the food and making these sorties in the direction of the stranger)

Based upon these criteria I suggest the following rating scale of rat "aggressiveness":

0. Pays no attention to (makes no overt movements in direction of) other rat or rats

1 Intermittently moves in direction of other, showing other signs of aggressiveness mentioned above (snorting, erect hair, etc)

2 More continuously aggressive, keeping constantly "after" other rat but not actually attacking him

3 Crowding against other rat, and standing over him when latter lies flat on back, but not biting him

4. Adds to above occasional attempts at bites

5 Bites other rat sufficiently hard to cause latter to squeal

6 Bites savagely and constantly, drawing blood (unless separated).

Note The influence of heredity upon traits described as 'wildness' and 'savageness' has been experimentally investigated by Yerkes and by Stone (using rats) and by Coburn (using mice) In general, the criterion of 'wildness' or 'savageness' used by all three of these investigators was the type of response made by the rat or mouse to the attempts of the experimenter to capture it and place it in his hand—a 'wild' or 'savage' rat or

mouse attempted to elude capture by running and by hiding while a 'tame' animal submitted readily to being picked up by the experimenter. The criterion of wildness or savageness was also based upon the animal's responses toward the experimenter, namely, the number and character of bites or attempted bites at the latter's hand and the like The method followed by the present experimenter differed from that used by the above investigators in that the rat's behavior toward his peers, rather than his reactions to the experimenter, is made the basis for the behavior-criteria upon which the rating scale herein suggested is constructed Just how 'general' the 'savageness' of Yerkes' and Stone's experiments and the 'aggressiveness' described in the present report may be is a matter meriting further investigation Recent observations have disclosed rats which attack the experimenter's approaching hand with a high degree of ferocity (getting a high rating for 'savageness' on the Yerkes or Stone scale) but which, when they are the 'stranger' rats in the cages of 'at home' rats, manifest zero 'aggressiveness' according to the criteria presented above

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University of California at Los Angeles, California

A STUDY OF PLEASURABLE EXPERIENCE AS A FACTOR IN COLOR PREFERENCE

RUTH STAPLES AND WILLIAM E WALTON

A number of investigators have suggested social factors such as cultural association or social conditioning to be of major importance in determining the color preferences of individuals (4, 6, 8, 9, 12, 13) This conclusion has usually been reached through an analysis of the reasons for color preferences as stated by the subjects, or it has been offered as the best explanation of the age, sex, and racial differences in preference or reactions to color The present study has been undertaken to determine experimentally whether or not pleasurable experiences in the presence of a particular color may be a factor in the preference of individuals for that color

Since the color preferences of young children have been shown to be less marked than those of school children and adults (10), nursery-school children were selected for the subjects of this investigation, presupposing that with them preferences might be more easily established, or existing preferences modified, than with older subjects The experiment was conducted

upon 5 boys and 8 girls. The age range of these children was from 3 years, 1 month, to 4 years, 10 months, with a mean age of 48 5 months The range of intelligence quotients was from 100 to 14+, with an average of 113

Procedure

A preliminary test was given to the subjects to determine their existing color preferences. Lights, rather than pigments, were used as the medium for presenting the colors since it is possible to regulate the brightness of lights, thus avoiding any preferences for color on the basis of brightness rather than hue. The apparatus used for this purpose, the chromo-pathometer, has been described elsewhere by one of the authors (11) The procedure with the children was as follows Each child was tested individually He was taken to a slightly darkened room and seated on a chair, 4 feet in front of the two windows of the chromopathometer The four colored lights, red, yellow, green, and blue, were then presented by the method of paired comparison With each pair of colors, the child was asked to point to the light he preferred After an interval of a few minutes the lights were presented again with the positions of the colors reversed Each color was then rated according to its affective value for each subject in terms of percentage of times chosen of the total possible choices for that color

Next, one color was selected for each child with which he was to have pleasurable experiences (This will be referred to as the "assigned color") This assigned color was, in each case, the one which in the preliminary experiment was shown to be the least preferred, or one for which the child had not shown marked preference Four of the children were assigned to yellow and three each to red, green, and blue. In order to include all four colors in the investigation, the original affective value of the color assigned was not the same for each subject

A series of pleasurable experiences with the color assigned was arranged in the following manner The subject was seated facing the chromo-pathometer and the four colors turned on in rotation, two lights of the same color appearing at the same time With the appearance of each color the child was given a box to open and his attention called to the colored lights. If it was his assigned color, the box contained a gift, otherwise it was empty. It was assumed that receiving a gift on the appearance of lights of a particular color might cause the child to form a liking for this color as soon as he recognized its significance On the other hand, a mild disappointment might occur if the box was empty and set up an unfavorable reaction to the other three colors

The detailed procedure was as follows The child entered the slightly darkened room and seated himself at a large low table before the apparatus The switchboard regulating the lights was on another table, screened from

the subject's view Four cotton-lined boxes with easily removable lids were on this side table, one containing an attractive toy, a small cookie, or a small piece of candy. The others were empty First, one of the boxes was placed before the child with the following instructions "I am going to put this box here You musn't touch it yet. Perhaps there is something nice inside and perhaps it is empty First I will turn on the lights (two lights of the same color were switched on) Do you see the lights? Now you may open the box and see if there is anything in it" If there was a toy inside, the child was allowed to play with it as long as he wished, or to eat the cookie or candy In any case, the procedure continued in the following way The lights were turned off and the box and toy removed A second box was placed before the child with the following instructions. "Now here is another box You watch the two little windows and when the lights come on you may open the box Do you suppose there is something for you in this one?"

This procedure was carried out with each of the four colors, at each experimental period The order of presentation of the four colors was rotated to prevent the children from expecting a "present" by means of serial order Different toys were used to keep up interest The children learned the procedure very quickly and would watch the windows for the lights eagerly. There was considerable variation in the time required by the different subjects to recognize the light which accompanied the gift One child recognized it the second trial Three learned it on the third trial, one each on the fourth, the seventh, the eighth, the ninth, and the twelfth The other four gave no evidence of ever recognizing their color, although it is possible that they did Of these four, three showed considerable confusion in ability to name colors. Their average chronological age was 44 months as compared with an average age of 45 5 months for the children who succeeded in the recognition However, the subjects failing to recognize their colors had an average mental age of only 47 months as compared with an average of 58 6 months for the other children These facts may, in part, account for their reactions The experimental procedure was carried out either once or twice daily (A M. and P M) with each subject for a period of from 15 to 20 school days. The entire experiment extended over a three-weeks' period

Within one week of the completion of the associations, the following experiments were performed on each subject

1 A test to determine whether or not a pleasurable association had been set up with a particular color This was a duplication of the preliminary preference test for colored lights.

2 Two tests to determine transfer as follows. (*a*) Preferences for colored papers Colored papers 4x6" were selected in the four primary colors matching as closely as possible the colored lights These were presented

by paired comparison as in the case of the lights and the general procedure duplicated (*b*) Preferences for colored blocks Four brightly colored blocks, red, yellow, green, and blue were selected exactly alike excepting for color These were presented in the same manner as were the colored papers No two of the above tests were given in any one day.

3 A test to determine the duration of the preferences Twelve of the subjects were available for a retest for preferences for colored lights after an interval of five months

4 A color-naming test, consisting of the four primary colors and orange, pink, violet, brown, black, and white This was included since it was thought possible that the knowledge of color names might be a factor in forming the associations with the colors

Results

The percentage of choices given by each subject to his assigned color was estimated for each color preference test as follows (*a*) the original preference for colored lights, (*b*) the retest after conditioning, (*c*) transfer to papers, (*d*) transfer to blocks, (*e*) retest after interval The average percentage of choices for the assigned color was then estimated for the entire group of children for each of the above tests. Since the number of subjects was small, Fishers' *t*-function (1) was used to determine the reliability of the differences between the averages of the various color preference tests as stated above. A summary of the results is presented in Table 1.

TABLE 1

Comparison of Percentages of Choices of Assigned Colors Before and After Pleasurable Experiences

All cases averaged N = 13*	Original preference	Preference after conditioning	Transfer to paper	Transfer to blocks	Preference after interval
Preference for assigned colors in terms of percentage of times chosen	34 6	82 0	70 8	64 9	45 8
ΣD^2	5678	.6933	5218	2125	3960
†value of "t" comparison with original preference		9 386	7 835	11,796	2 786

*Only 12 subjects were available for the transfer tests and for the retest after a five-month interval.

†The differences are significant if *t* is equal to or greater than 2 201

Considering the averages for the group, there is a decided increase in the strength of preference for the assigned color after the pleasurable experiences as compared with the original presentation of the lights (82% *vs* 34.6%). The affective values of the assigned colors drop in the case of the colored papers (70.8%) and drop still more with the colored blocks (64.9%). It will be remembered that the papers closely matched the lights in value, while the blocks varied somewhat. After the five-month interval there is a still greater drop in the affective value of the assigned colors (45.8%). However, it will be noted, the value of "*t*" is high enough to give unquestionable significance to the differences between the averages of the original preference and (*a*) the average for the lights after conditioning, (*b*) the average for the papers, and (*c*) the average for the blocks. In the case of the smallest difference, i.e., the color preferences after the time interval as compared with the original preferences the value of "*t*" is 2.786 which gives 99+ chances out of 100 that this difference is a true one.

A consideration of the individual cases is of interest. Twelve of the 13 subjects showed a decided increase in the affective value for the assigned color after the associations, in 5 cases the choices being 100%. For one child the affective value remained unchanged. This child, a girl of 46 months with a mental age of 50 months, was one of the four who never gave evidence of recognizing the significance of her particular color (blue). She was able to name red, green, blue, white and black and violet correctly, pink she called "red" and yellow "light." She was ignorant of the names of brown and orange. She never paid much attention to the lights and showed the least interest of any of the children in the opening of the boxes. She chattered gaily throughout the tests, on a wide variety of subjects, opening the boxes incidentally. She was not available for the transfer tests. One subject showed slightly less preference for his assigned color in the case of the colored papers and the colored blocks than in the original test. This child named all colors correctly and had been an enthusiastic subject throughout the experiment. Two other children showed neither increase nor decrease in the preference for colored blocks as compared with the original preference for lights. In the final test, after five months, seven of the subjects still favored their assigned colors more than originally, three held them at the same value, and for two children the affective value had decreased slightly. Both the ability to name colors and the intelligence of the subject, while affecting the ease with which the children learned the significance of the particular colored light, apparently were not factors in setting up the pleasurable associations.

The results of this study indicate that the pleasurable experiences with the assigned colors very definitely influenced the subsequent color choices of the subjects, whether the colors were presented in the form of lights, papers, or blocks.

A comparison of the different colors is also of interest, although the cases are too few to suggest more than *possible trends* *Dividing the children* into four groups representing the four assigned colors, we find that the original preference for red for the group assigned to red was 50% of the total possible choices for this color For the group assigned to yellow, yellow originally held a 33% affective value Green and blue rated lowest with the two groups assigned to these colors—with 28 and 27% respectively After the conditioning experiment red jumped to 94% of the possible choices of the group assigned to red, yellow to 79%, green to 89%, and blue to 67% Coming to the retest after five months, we find red back to its original value of 50%, yellow to 38%, green to 42%, blue at 56% The greatest permanent gains appear to be with green and blue, the colors with the least original affective value. This is of especial interest, since evidence has been presented elsewhere that in infancy red and yellow are strongly preferred and much more effective in eliciting responses than blue and green, but that, with increasing age, blue and green gain in value (2, 5, 7, 3, 10) It is possible that the earliest color preferences have a physiological *basis, but that, as development progresses, social conditioning becomes the* major basis for preference

Summary

Thirteen children were given a series of pleasurable experiences accompanying the presence of lights of a particular color After these associations the preferences of the subjects for colored lights, paper, and blocks were determined and compared with their color preferences previous to the experimental procedure After a five-month interval, the children were reexamined for color preferences.

The pleasurable experiences with a given color resulted in a decided increase in the affective value of that color, as evidenced by the children's preferences for colored lights, blocks, and paper These preferences were maintained to a lesser but significant degree after a five-month interval, somewhat more pronounced in the cases of blue and green than with red and yellow

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University of Nebraska
Lincoln, Nebraska

The State University of Iowa
Iowa City, Iowa

BOOKS

KNIGHT DUNLAP *Habits Their Making and Unmaking* New York Liveright, 1932. Pp x+326

The publication of a book by Knight Dunlap should arouse anticipations of a work marked by unusual precision and care For, as is well known, there are in psychology few more fervent preachers of scientific spirit and method, and few more uncompromising critics of other writers In the case of this book, furthermore, a preliminary survey of its contents heightens whatever favorable anticipations one may have had The chapter-titles are well chosen Also, despite the simple essay style of the book, one finds that it concludes with a classified bibliography of about 600 titles Naturally, a bibliography of such unusual proportions leads one to expect that the discussion will reflect an unusual quantity of careful study and thought

The preface reinforces one's hopes It describes the purpose of the book as, first, the integration and reinterpretation of the experimental work on learning (especially as regards the interrelations between learning and unlearning) and, second, the application of the resulting principles to problems of emotional and social habit The preface describes the book as non-technical, but implies that professional psychologists will find material of interest, especially in the first half of the book The author states "I suppose that my treatment of the psychology of learning will seem to many readers to be radical, even revolutionary It will be found that I have discarded the conventional formulations and flouted the 'principles of learning' set forth in widely used texts . . . the revolt is directed . against the theories and the traditions which originated before the experimental era of psychology, and which need to be swept away in order that the experimental results may be understood" (pp. viii-ix)

However, the detailed study of the book does not bear out one's hopes In fact, one is tempted to say that in general the book is marked by those same faults which Dunlap criticizes so caustically in others. The book presents very little factual material, contains only a few scattered theoretical points which merit favorable comment, and displays so little contact with the experimental literature on learning that one is forced to conclude that the bibliography (which, incidentally, was contributed by Dr Willis C. Beasley) was prepared as an *ex post facto* measure, and not as an aid for the writing of the book

The more specific discussion of this book will be divided into three parts I, a summary of the general contents of the book, II, a discussion of its main defects; and III, a discussion of its main good features

I

The general contents of the book may be indicated very briefly The first chapter gives a general description of psychological processes, and of habit in particular It introduces the central theme of the book—the principle that the responses made during learning typically change radically as learning proceeds The second chapter, "The Fundamental Principles of Learning," distinguishes four different types of response on the basis of the relation between thought and action in each, and shows that each type is modifiable through learning. This chapter ends with arguments for the view that instincts and reflexes are learned responses The brief third chapter deals almost solely with the definition of voluntary action Chapter IV, on "Psysiological Theories of Learning," criticizes, first, the specific-path theory of habit, and, second, the claim that conditioned-response studies have contributed any new interpretation of learning Chapter V presents the book's most detailed analysis of the forms of learning This analysis is used in this chapter as a basis for re-discussing Dunlap's "beta hypothesis,"[1] but elsewhere in the book this analysis receives no mention Chapter VI is a summary of conventional material on factors influencing speed of learning Chapter VII, "Retaining, Recalling and Relearning," is devoted almost exclusively to the description of norms of learning, means of measuring retention, etc Chapter VIII deals with the distinction between memory and the simpler forms of retention, and, secondly, with the so-called systems for improving memory. Chapter IX gives an elementary description of the different principal varieties of maladjustment (physiological and structural maladjustments, feeblemindedness, etc) Chapter X repeats, with virtually no factual or theoretical additions, Dunlap's previous brief reports on his clinical use of "negative practice" Chapter XI is principally an analysis of the nature of emotion and emotional habit The final chapter presents arguments for the view that intelligence tests measure knowledge and skills, but not innate potentialities

II

Turning now to the discussion of the defects of the book, we find the following

A. The book contains various errors of fact The following may be mentioned (1) "The iris reflex   does not seem to be modifiable except

[1]Unfortunately, the author has shifted his definitions in this connection The definition formerly given of the beta postulate now is given as the definition of the gamma hypothesis, and the definition formerly assigned to the gamma postulate now is given as the definition of the beta hypothesis (compare pp. 78-80, 94-95 of the present volume with Dunlap's statements in *Science*, 1928, **67**, p 360, and in the *Scientific Monthly*, 1930, **30**, p 66) The author gives no explanation of why this change has been made, and perhaps it is merely an oversight

through changes in conditions of health" (p. 24). This statement appears in a paragraph which cites the disappearance of the Babinski reflex and the conditioning of the blink reflex as examples of reflex modification Accordingly, on the iris reflex, Dunlap disregards the studies by the Shermans (1925) and by Cason (1922) (2) In discussing studies of guidance, Dunlap says. "Guiding the animal should produce important results We might, for example, lead an animal through a maze It would be difficult to guide a rat, but one can easily guide a dog or goat It has not been experimentally done" (p 107) As a matter of fact, Alonzo (1926) performed this very experiment, first having accustomed his rats to being led by leash. What is more, this study by Alonzo is listed in the bibliography as one of the five references cited on this section of Chapter VI. (3) "With rats not only does the learning of one maze or one problem box . seriously inhibit the learning of another maze or another problem of the same type but different details, but even the learning of a maze, far from facilitating the learning of a different problem . . more often actually inhibits the second learning" (p 121) However, on the contrary, studies of transfer of training in rats all report positive transfer except in cases where the conditions are such as to produce simple habit interference Thus, see Webb (1917), Wiltbank (1919), Dashiell (1921), Hunter (1922), Ho (1928), and Jackson (1932), as well as earlier studies by Watson, Richardson, and Bogardus and Henke In fact, positive transfer is so real and apparent that it is fast becoming standard maze technique to give preliminary training of some sort in order to get a more capable and standardized performance on the maze (4) "Even when . . . the relearning shows no saving over the previous learning, the recognition of the materials as something learned before clearly indicates retention " (p 142) This statement similarly conflicts with experimental data Thus, Ebbinghaus (whose work Dunlap regards highly enough to have chosen for the *Psychology Classics* the chapter on learning of his *Grundzuge der Psychologie*, "1813" edition [p 270— probably the 1905 edition is meant]) found savings scores of about 33% with nonsense syllables, despite the fact that "When the series were repeated 8 or 16 times" (in the original learning) "they had become unfamiliar to me by the next day Of course, indirectly, I knew quite well that they must be the same as the ones studied the day before, but I knew this only indirectly. I did not get it from the series, I did not recognize them" (p 58, *Memory,* translation by Ruger and Bussenius) Scott (1930) has found the same phenomenon in maze learning (5) Dunlap credits Ebbinghaus with having originated the concept of the intelligence quotient (p 262). The correct reference is, of course, to Stern (6) In discussing the standardization of intelligence tests, Dunlap comments as follows: "It is somewhat amazing to the psychologist to find investigators naively attempting to determine whether or not intelligence actually remains constant from year to year Such investigators, obviously, have failed to

understand the plan of construction of the tests, and the method of standardization Failure to find constancy of the proper numerical index of intelligence from year to year (for the averages of large groups, not for individual children) would merely show that the lists were imperfectly constructed or inadequately standardized" (p 262) However, to others it may seem that the amazing thing in this matter is that Dunlap should describe the problem of the constancy of the IQ as having been commonly treated as a matter of the "averages of large groups" (the parenthesis in the above quotation is not an insertion of the reviewer) This problem of constancy of IQ is, in truth, often too loosely treated, the error being made of not distinguishing between variations resulting from errors of measurement and variations resulting from changes in the trait measured, but the investigators have not made the crude error which Dunlap charges against them.

The above errors would not be worthy of mention, perhaps, if a large quantity of experimental material were covered by the book, but such is not the case As remarked above, considerable portions of the book are occupied with the elaboration of purely logical material, such as on the topics of what constitutes voluntary action, what the different possible norms of learning are, etc

B The book contains many unsupported, and in many cases unreasonable, dogmatic pronouncements The following examples may be cited: (1) "For the greater part of the insane, it is not evident that they are shorter lived or less happy than the 'normal' person But they are non-productive, they require our support, and in other ways they annoy us and interfere with our plans This is the essence of insanity The type *of thought processes does not matter The 'mind' of the average lunatic* is probably as orderly as that of the average 'sane' man" (p. 181) (2) " doubtless many" (morons, in the technical sense of the word) "have attained fame in art and literature by concentrating their efforts on a few simple techniques" (p 183) (3) "Thumb-sucking . . is a sign that the child has not been adequately socially stimulated The baby should be allowed to amuse itself only for short and carefully controlled periods It should be amused, that is, socially stimulated during practically all of its waking time. Fortunate is the infant whose mother has time to entertain it, and whose efforts in this line are supplemented by those of grandparents, uncles and aunts" (pp 213, 215) (4) "The girl or married woman who has engaged in illicit sex relations usually does not regret or repent of her conduct at all She may, however, become neurotic through constant fear Men are the conspicuous repenters of sexual conduct " (p 236) (5) "Psychologists are familiar today with the numerous cases 'treated' by psychoanalysis and left in a condition in which the maladjustments are not only more serious, but more difficult to remove than in the patient's original condition Suicide following psychoanalytic treatment has been

frequent" (p. 193). Particularly for this last statement would it be desirable to have proof. If true, the assertion is most important, both practically and *theoretically.* And since "psychologists are familiar today with the numerous cases . . ." it should not be difficult to provide the evidence.

Such assertions as the above are the more noticeable because of their contrast with the book's many expressions of caution. An example of the latter is the statement, "We assume . . . learning operates by producing organic changes, perhaps in the nervous system . . ." (pp 13-14). The change of the author's attitude is certainly rather marked. "Perhaps," Dunlap says—not "almost surely," not even "probably," but "perhaps in the nervous system"! It does seem a rather puzzling thing that the one group of statements displays such breezy confidence and the other group such overwhelming caution, especially in view of the fact that the adequacy of evidence seems inversely related to the degree of assurance. Of course, the book probably will have a greater popular appeal because of this tendency to be ultra-conservative on topics where competent workers generally make confident assertions, and to be daring and dogmatic on topics where others generally feel that it would be unsafe to hazard opinions; but this characteristic hardly increases the scientific value of the book.

C. The book is not careful in its terminology. This defect is not ubiquitous—at a few points there is a care in definition which could well serve other psychologists as a model for imitation. But such care is not the rule. In the first place, although Dunlap insists that established terminological usages generally should be respected (see pp 5, 18, 49), he occasionally disregards this precept with no apparent warrant. For example, he states: "By stammering we usually indicate any defect of speech which is not due *to anatomical faults, but is an actual speech habit" (p 197). (Stammering, then,* covers lisping and all other habits of mispronunciation!) The term "neurotic" is redefined as designating only those persons who are not only inefficient or unhappy, but who are also socially obnoxious (p. 188). "Imagining" is used repeatedly to refer, not to thought of unreal or unknown situations, but to the simplest of all sorts of thinking—that type, whatever it may be, which maybe constitutes the minimum thought requirement for human and perhaps also for animal learning (p. 37, etc.)

In the second place, Dunlap uses even some of his own definitions inconsistently. Thus, near the start of the third chapter, he states: "Responses which the individual performs in spite of the fact that he desires not to make them, or chooses or decides not to make them, we usually designate as *involuntary.* Still other acts, which apparently are performed without any desire or choice, for or against them, we designate as *non-voluntary*" (pp. 43-44). Yet, although he nowhere redefines "involuntary" and "non-voluntary" in any other sense, he states within the page: "The action of the salivary glands is, for the greater part of the time, involuntary, . . . so is the action of the arm and leg muscles", and he also states that the

problem of stutterers is a matter of finding ". . . means by which certain acts shall be made voluntary at certain times and be allowed to proceed involuntarily at other times" (p 44). Another example is that, after having used "feeling" to refer to emotional responses (Chapter I) and to the affective part of emotional responses (Chapter XI), Dunlap makes the following statement without integrating it at all with the rest of the discussion "A feeling, in any useful meaning of the term, is a bodily state, existing in the muscular and connective tissue, and capable of stimulating nerve endings" (p 241). The most serious instance, however, of Dunlap's changing of meanings occurs in his use of his very important expression *"negative practice"* Dunlap generally uses this term (and, in fact, specifically defines it, pp 94-95) as referring to practice in which the learner deliberately repeats the response which he wishes to eliminate, thinking at *the same time of the fact that the response is one which he wishes to* eliminate But, at a few points, Dunlap employs the term in this altogether *different sense. In speaking of learning to throw darts, to type, etc, he says,* ". . . negative practice begins the learning process and positive practice *finishes it"* (p *227, elaborated further on* p *228)* It is of course clear that in such early trials the learner is not *deliberately* making the mistakes, but makes mistakes *only in spite of his intention of making correct reactions* Since such practice is quite appreciably different from that which Dunlap has generally designated as negative practice, it seems that such shifting of meaning of this term cannot fail to breed confusion

D The book is not a unified whole This is true particularly as regards Dunlap's discussions of the question of the forms or types of learning This topic is handled in three different ways in different parts of the book, and *almost no attempt is made to integrate these different treatments* This defect of the book can hardly be illustrated adequately in any brief fashion One minor example may be given, however The second chapter opens thus. "We learn by acting, by perceiving and by thinking So far as we can now determine, no other ways of learning are possible" (p 18) This sounds definitive But on the following page Dunlap states "The distinction between thinking, perceiving and 'acting' is not so sharp as it might appear to be. Action-responses may be perceptual Other action-responses may be processes of thinking . Still other action-responses are theoretically possible in which there is neither thinking nor perceiving" From this statement, and particularly from the "theoretically" in the last sentence quoted, it appears that within two pages Dunlap has become very dubious of the idea rather clearly implied by the chapter's opening sentence, that at times we learn by "acting" in a sense distinct from learning by perceiving or thinking Still further, other statements are made which seem to cast "learning by perceiving" into equal uncertainty Thus, pp 29-30. *"It is not evident, though it may possibly be true, that we can learn without* thinking . if it turns out that learning occurs in human beings only

through thinking, the extension of the rule to the lower animals would be in order " Because of such statements as these one cannot tell what is the net view of the chapter

E The book is marked by serious omissions It is not merely that the book leaves untouched a wide range of material that would be essential for any well-rounded treatment of learning, for such a well-rounded treatment would be impossible in a book of this length, even though it were very tersely written The defect consists rather in the fact that the book omits important material on topics which the book specifically professes to treat For example, since the occasion for this book springs from Dunlap's clinical work with the negative-practice technique, one naturally looks for definite information about his actual experience with this method But, actually, the 38-page chapter on "The Breaking of Specific Bad Habits" contains virtually no new material to eke out the two slender preliminary reports in *Science* (1928) and the *Scientific Monthly* (1930) The suggestion is given that some cases demand treatment over periods of months, that relapses may occur, and that more conventional psychotherapeutic methods may well be used to supplement the negative practice, but there is still no mention of how many cases Dunlap has studied, what the particular nature of these cases was, what percentage of success has been achieved, what procedure is used in certain cases, etc This is disappointing, for Dunlap's work on habit-breaking has been a really stimulating contribution and has aroused hopes that his technique would prove to be an unusually valuable therapeutic tool

No treatment is attempted of the theoretical problems of the operation of negative practice, and, in fact, Dunlap forswears the attempt (p. 314) A few theories, however, might well have been mentioned, such as Woodworth's suggestion that negative practice permits the functional isolation of the undesired habit and the establishment, by means of bringing the effect of the response into close proximity with the isolated response, of a conditioned inhibition of the undesired habit (Woodworth's *Psychology,* 1929 edition, pp. 177-178).

Since the preface introduces the book as aiming to integrate the experimental literature on learning and unlearning, one might legitimately expect some discussion of negative adaptation, of experimental extinction in conditioning studies, of changes of response that occur when new avenues of response are opened up to an animal which already has developed some more indirect solution (see studies from Tolman's laboratory), of dissociation (especially in view of Dunlap's expressed admiration for Janet's work), of repression, of retroactive inhibition, and of the problem of how such habits as tics and compensatory reactions arise. But of this assortment of topics only repression and retroactive inhibition are mentioned (p 212 and pp. 156-157, respectively), and even these are but barely mentioned Further, despite the space devoted to the reconsideration of the beta hypothe-

sis, no reference is made to the large number of studies by Thorndike and others which have demonstrated, even more extensively than Dunlap's own work, that repetition is only an inconsequential factor, if any factor at all, in causing learning

It would have been interesting if Dunlap had developed his suggestion that skills and many of our knowledge-habits are to be understood as being perceptual habits (see pp. 19-22, 74, etc). A considerable body of evidence on the modification of perception by training has been gathered in psychology, but this material is at present very inadequately utilized in our general theory of learning Dunlap speaks at a few points as though he were planning to develop this notion, but the book ends with the task undone

III

Enough has been said about defects We may turn now to a brief summary of the features of the book which deserve favorable notice They are:

A The principle which forms the central theme of the book—that the learner's response typically changes radically as learning proceeds This point is, of course, by no means novel (cf the descriptions, from Small on, of how the behavior of the rat changes on successive maze trials, etc), but it is nevertheless true that we have frequently overlooked this point in our theoretical discussions of learning. This old oversight is, of course, being corrected by other workers, and often with considerably more experimental material than Dunlap has contributed (see Thorndike's *Fundamentals of Learning*, and also the writings of Kohler, D. K Adams, Muenzinger, etc), but the fact remains that Dunlap is lifting his voice in a well-needed chorus.

B Dunlap's criticisms of the specific-path theory of learning (pp 56-58) This treatment too would have been stronger if it had summarized the experimental literature on the problem, but Dunlap does give these two important logical points first, that the theory presupposes that learning comes through repetition of the response to be learned, whereas often the response changes radically during the learning, and, second, that if habits did depend upon certain bonds of lowered resistance between successive neurons, neurons involved in one habit would thereby be rendered unavailable for any other habits, since each neuron, whenever excited, would pass on its nervous impulses along the one path which was mechanically fixed. As Dunlap says, this principle is inconceivable, since the number of sensory and motor neurons is so limited in comparison with the diversity of habits that depend upon any particular sensory surface or motor system Dunlap's statements in this connection carry him close to the Gestalt principle that there must be some sort of dynamic organization to central nervous activity, but, like much relevant material at other points, the principles of Gestalt psychology pass without mention

C Part of Dunlap's discussion of conditioned-reflex work, particularly

his consideration of conditioned-reflex doctrine as a distinctive theory of learning (pp. 59-70) Dunlap charges that there has been much looseness of discussion in the interpretation of conditioning; but, running through the general structure of Pavlov's work, he thinks one can discern the fundamental assumption that the nervous processes involved in conditioning consist of an aggregation of discrete reflexes of the type that might be secured if one operatively removed all parts of the nervous system except the barest elements of a reflex arc The compounding together of such abstracted entities would constitute conditioning, etc To this theory, Dunlap objects that no such units of activity exist except in the realm of artificial abstractions, that actual reflexes involve quite wide coordinations and interactions, and that complex activities, since they can be performed under considerable variation of stimulus and response, cannot be mere mosaics of reflexes This is good doctrine The same ideas have been expressed earlier by Piéron, Lashley, Kohler, and others, but these ideas, too, still deserve emphasis

D. The book's emphasis of the fact that skills are often established partly through the operation of knowledge-habits (e g, through recalling instructions on how to respond), even though this knowledge element may drop out after the skill has been established (pp. 74-76, 153)

E The idea which is coming to be expressed widely by psychologists today, that emotions (in the sense in which this term has generally been used) are not matters merely of feeling or affect, but are *feeling plus ideational content* (pp 237-247, 317). Dunlap claims that we identify emotions more by their thought content than by their feeling element, and that emotional responses with quite different feeling content are generally classed as the same emotion if they possess the same thought content.

F The thought which forms the central topic of the last chapter, that intelligence tests do not measure directly the innate potentialities of a person, but, rather, that they permit estimations of this only when there are present the very-rarely-achieved conditions which would make possible a proper allowance for any atypical learning opportunities which an individual had had It seems certain that we need more emphasis of this point, for we doubtless generally have been guilty of wishful thinking in this field—we would like so much to know how different races compare with respect to their basic potentialities, how different nationalities compare, how different social or regional groups compare, etc, that we have rushed in and measured and generalized, when, in truth, we had no means of knowing what corrections to make for differences of environmental opportunity. This chapter is perhaps a bit extreme in some ways, and, as noted before, it contains a few errors, but as a whole it is a chapter which the reviewer heartily applauds

G. The section of the appendix (pp 308-313) devoted to summarizing the criticisms which Dunlap has offered several times previously of the

psychoanalytic concept of the unconscious This section by no means disposes of the psychoanalytic concepts of unconscious mental activity, for many of these concepts are questions of fact, not of definition, and Dunlap does not deal with them on this level But there are some suggestions here worth pondering on

II The bibliography It is not exhaustive on any topic, and does not pretend to be But as initial material on many topics, this bibliography may perhaps prove useful to psychologists—at least to those who do not want to use the more detailed bibliographies of Hunter's and Lashley's chapters in *The Foundations of Experimental Psychology,* Robinson's and McGeoch's reviews of *Memory* and of *Skill* in the *Psychological Bulletin,* and McGeoch's recent 1200-item bibliography on human learning in the January, 1933, *Psychological Bulletin*

One last comment might be made apropos of the general evaluation of Dunlap's contributions to learning The present book tends to create the impression that other psychologists have generally held the view that learning comes about through the repetition of the act to be learned, and that Dunlap's work has been the first effective challenge of this old doctrine The reviewer is inclined to accept a somewhat more conservative view It is only in some of the theoretical discussions of learning (and particularly, when under the spell of the theory of paths of lowered synaptic resistance) that we have very often forgotten about the fact of change of response with practice For such theoretical discussions Dunlap's work has had a vitally clarifying influence In the discussion of all other phases of learning, however, it has been tacitly or explicitly assumed that the response changes as learning proceeds, and that *the way to bring about these changes as rapidly as possible is through constant effort on the learner's part to make his response conform to some standard or ideal* It does not appear that this fundamental principle has been altered at all by the contributions which Dunlap has made—at least to date his work indicates merely these two points (1) that in certain special cases where erroneous habits have been set up, learning may be facilitated by a digression to dispose of these unusual interfering factors, and (2) that sometimes the most effective way to dispose of such erroneous habits is by the special technique of negative practice which he has developed.

Robert Leeper

University of Chicago
Chicago, Illinois

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THE NORMAL SENSORY CONTROL OF THE PERFECTED DOUBLE-ALTERNATION SPATIAL-MAZE HABIT OF THE ALBINO RAT*[1]

From the Psychological Laboratories of Clark University

BARRY CASPER

I INTRODUCTION

The widespread belief that all learned behavior, including the most complex acquisitions of human beings, can be explained in terms of the theory of conditioning requires a more solid experimental foundation The theory is based mainly upon results obtained with relatively simple behavior of such animals as the rat and the dog. One method of testing the general validity of this theory is to determine whether or not the more complex behavior of these animals is explicable in such terms. Thus, one might raise the question, "Does the ability of the albino rat to traverse the maze without error present characteristics which are compatible with the theory of conditioning?" A primary step in the attempt to answer this question is the identification of the stimuli for those responses which constitute the perfected maze habit The identification of the precise stimuli involved is a task that has proved too difficult for investigation, but the problem of the determination of the sensory fields or modalities in which these stimuli lie has been attacked with considerable success It is with certain phases of this problem that the present investigation is concerned.

At the present time three conflicting beliefs are held regarding the extent to which the various sense modalities are involved in the maze situation One view, based mainly on the work of Watson (24) and Carr and Watson (5), assumes that proprioception is the only form of stimulation necessary for the learning of the maze, and that the perfected maze habit is controlled by proprio-

*Accepted for publication by Walter S Hunter of the Editorial Board

[1] This study was carried out under the direction of Professor W S. Hunter The author wishes to express his indebtedness to Professor Hunter for constant aid and helpful criticism in the prosecution of this research and in the preparation of this manuscript.

ception Another view, exemplified by that of Lashley and Ball (15), assumes that no sensory processes are necessary for the performance of the completed maze habit, but that this behavior is controlled by a "wholly central neural mechanism" The third view, exemplified by that of Hunter (13), is that both the formation of the maze habit and its execution after mastery are dependent upon a multiplicity of stimuli and receptor modalities

The existence of these diverse views is based upon an over-simplified conception of the general problem of the sensory control of the maze habit The results upon which these beliefs are based, although different, are not necessarily contradictory, because different problems within the general field have been investigated. The general problem of the sensory control of the maze habit may be divided into the following specific problems (1) Which sense modalities are *normally utilized* in the *process of learning?* (2) Which sense modalities are *necessary* for *learning?* (3) Which sense modalities are *normally utilized after the maze has been learned?* (4) Which sense modalities are *necessary* for errorless performance *after the maze has been learned?*

Two assumptions are implied in this classification. (1) The sense modalities involved during the process of learning may be different from those involved after mastery, i. e., during the progress of learning a shift in the sensory control may occur (2) A number of modalities may be involved during the process of learning or after its completion; but, in the absense of one or more of these modalities, sensory guidance through the remaining modalities is adequate for a normal learning rate or for errorless performance after mastery. The work of Bogardus and Henke (1) and Vincent (22) supplies some experimental justification for the first, and the work of Dennis and Porter (7), for the second, of these assumptions

We believe, then, that the apparently contradictory conclusions drawn by previous investigators in the field of the sensory control of the maze habit have really been answers to different questions Which question was answered depends upon the method that was employed The method (used most frequently) of determining the effect upon the rate of learning, or upon performance after mastery, of the elimination of *individual* modalities supplies data only for the problem of which modalities are *necessary* (or

unnecessary) To determine which modalities are *normally* utilized, when all are available, it is necessary to investigate the effect of the simultaneous elimination of various combinations of several modalities, to avoid the possibility that, in the absence of one modality, dependence upon those which remain will mask the extent to which this modality is utilized.

In the light of this analysis, it is necessary to re-evaluate the contributions of previous investigations to the fourfold problem of the sensory control of the maze habit Classifying these investigations according to the specific problem to which they contributed, we find the following

1 *The sense modalities necessary for the acquisition of the maze habit, or for errorless performance after mastery* In general, the elimination or reduction of the possibility of receiving either visual, olfactory, auditory, cutaneous, or proprioceptive stimulation, *alone,* has not prevented animals from acquiring the maze habit, or from re-acquiring it if the habit was disturbed This conclusion is based upon the work of Watson (24) (vision, audition, olfaction, and cutaneous sensitivity), Carr (2-4) and Lindley (17) (vision and olfaction); Lashley and Ball (15) (vision and proprioception); Liggett (16) (olfaction); Weaver and Stone (25), Hunter (11), and Dennis (6) (vision), and Dorcus and Gray (8) and Ingebritsen (14) (proprioception). Walton (23) found that rats could master a maze in which the sole basis for correct choices was a difference in visual brightness Honzik (10) found that a maze could be mastered in the absence of all constant exteroceptive stimulation. After a high degree of accuracy had been reached in a maze whose units were constantly interchanged, parts of the maze were eliminated by the use of short-cuts. The facts that Honzik's animals made errors in the units beyond the short-cuts and that carrying the animal to the part of the maze beyond the short-cut increased the number of errors, however, leave open the possibility that the degree of accuracy shown in the short-cut trials was dependent upon proprioceptive stimulation resulting from the movements of the animals from the starting-box to the beginning of the short-cuts and from exploratory movements through the early parts of the maze beyond the short-cut.

2. *The sense modalities normally involved in the acquisition of the maze habit* The work of Vincent (21) indicates that

vision and cutaneous sensitivity are utilized in the elevated-maze habit The work of Bogardus and Henke (1) and Dennis (6) indicates that cutaneous sensitivity is utilized in the standard type of maze habit The work of Liggett (16) and Lindley (17) indicates that olfaction is utilized. Of special interest for our suggestion that the effect produced by the elimination of a single modality is not a satisfactory measure of the extent to which this modality is normally utilized are the results of Vincent (21), Carr (4), and Lindley (17) Vincent (21) found that the retardation of learning produced by the simultaneous elimination of the eyes and the vibrissae was much greater than the sum of the retardations produced by the elimination of the eyes and the vibrissae individually This suggests that the eyes and the vibrissae were involved to a greater extent than the effect produced by the elimination of either alone would indicate, i e., that, in the absence of one modality, dependence upon the other masked the extent to which the former was utilized Similarly, Carr (4) and Lindley (17) found that the elimination of vision alone had no effect upon learning performance, but that the elimination of both vision and olfaction produced a much greater disturbance than did the elimination of olfaction alone. This suggests that the elimination of vision alone did not retard learning because *both* vision and olfaction were normally utilized, and, in the absence of vision, the animals utilized olfaction The results of these three investigators strengthen our belief that the absence of an effect following the elimination of a single modality is not satisfactory evidence that this modality is not normally utilized.

3. *The sense modalities normally utilized after the maze has been mastered.* The work of Watson (24), in which one of four normal animals who had been trained in the light was badly confused when tested in darkness, indicates that at least some animals utilized vision. The disturbance of his animals by the rotation of the maze indicates that some undetermined exteroceptive modality may have been utilized. The fact that Carr and Watson's animals (5) were disturbed by shortening and lengthening of the maze suggests that some sense modality, or modalities, were utilized. Whether these were exteroceptive or proprioceptive was not satisfactorily determined. The work of Carr (2-4) suggests, upon the basis of the analysis presented in the preceding paragraph, that

vision and olfaction were utilized. The work of Higginson (9), *Valentine (20), and Patrick and Anderson (18) indicates that* some undetermined exteroceptive modalities were involved. The results of Lashley and Ball (15) and Tryon (19) indicate that vision was utilized The results of Hunter (12) and Honzik (10) indicate that some non-visual modality, or modalities, were utilized by blind animals on the elevated maze.

In summary, the following general statements may be made concerning the contributions of previous investigations (1) No single sense modality is necessary for the acquisition of the standard maze habit (2) Only rarely is an animal dependent upon any single modality for errorless performance after mastery. (3) Vision, olfaction, and cutaneous sensitivity probably are normally utilized in the acquisition of the maze habit. (4) Some sense modalities are normally employed after the maze has been mastered. Their identity, and the extent to which each is employed, has not been satisfactorily determined, although the results point to vision, olfaction, and proprioception. (5) The effect produced by the elimination of a single modality is not a satisfactory measure of the extent to which this modality is normally utilized. (6) There is some individual variation in the extent to which individual modalties are normally utilized, depending, presumably, upon the sensory acuity of the animal and the nature of the maze situation.

II. Problem

It is apparent that little that is definite is known concerning the extent to which each sense modality is normally employed after the maze habit has been acquired It is with this aspect of the general problem of the sensory control of the maze habit that the present investigation is concerned Specifically, the present study is an attempt to evaluate the extent to which the albino rat is *normally* dependent upon vision, olfaction, audition, and proprioception for errorless performance in the double-alternation spatial maze after mastery has been attained

III. Procedure

A General. The procedure employed in the present study was designed to test the theory that the normal sensory control of the maze habit is multiple, and that, after any one modality is elimi-

nated, dependence upon the modalities which remain will mask the extent to which the eliminated modality was utilized when it was available. Normal animals were first trained on a 9-unit double-alternation maze After the criterion of mastery was reached, vision, olfaction, and audition, singly and in combination, were eliminated by means of operations upon the animals In order to measure the extent to which the eliminated modalities had been utilized, the animals were then tested upon the same maze that had been learned under normal conditions. To test the possibility that those animals that were not disturbed by the elimination of particular single modalities were utilizing other modalities, additional modalities were eliminated from these animals, who were then tested again upon the maze. An additional group of normal animals was trained on the maze and was then tested on a maze of the same pattern, but from which it was believed that *all* constant exteroceptive stimulation was eliminated by means of rotation of the maze and by the interchange and rotation of its units after each trial This last group was used to compare the effect produced by the simultaneous elimination of all exteroceptive stimuli which may have been used as directive cues during original learning (by means of *manipulation of the animal's environment*) with the effect produced by the simultaneous elimination of vision, olfaction, and audition by *means of operations upon the animal*

B Apparatus Small wooden boxes, 15″ long, 4″ wide, and 4″ high, served as individual living-cages The bottom, side walls, and rear wall were constructed of wood. The top was covered with ¼″ wire mesh, hinged at one side. The floor consisted of ½″ wire mesh. A removable metal pan was placed between this wire floor and the wooden bottom to receive the animal's excretions A wooden door in the front part of the cage rested in vertical grooves and could be moved in the vertical plane.

A long runway, 81″ long, 4″ high, and 4″ wide, was used for preliminary training trials. The wooden sides and floor of this runway were painted a dull flat black The top was covered with ½″ wire mesh Doors to prevent retracing (which were employed in the maze) were not used in the preliminary runway

The 9-unit T-shaped double-alternation maze shown in Figure 1, Maze I, was constructed of a series of identical separate units. (In Figure 1, the rectangle *HIJK* represents one unit) The turns

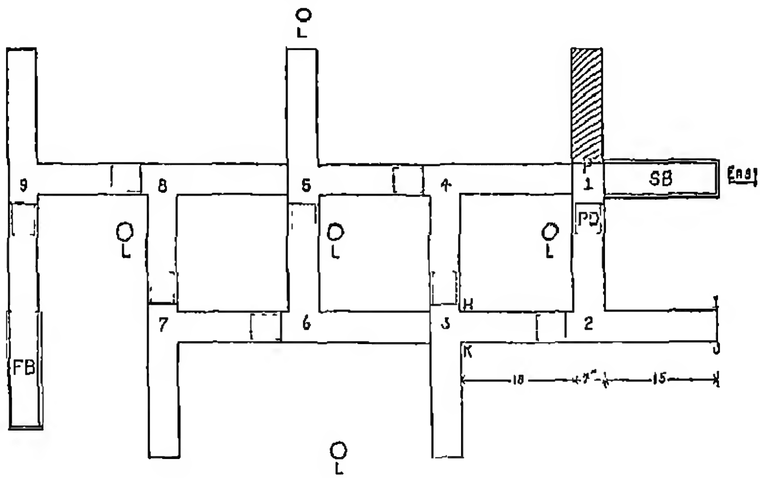

FIGURE 1
PLAN OF MAZE I

SB—Starting-box
P—Partition which shut off shaded portion of first unit of maze (the shaded portion of the first unit was not part of the maze)
RD—Doors used to limit retracing (shown raised to top of maze)
L—Lamps which illuminated maze
FB—Food box
HIJK—One unit of the maze (the second)
1, 2, 3, etc indicate the various units of the maze

required after the maze had been entered were: right, right, left, left, right, right, left, and left The distance to be traversed between each turn was 15". The wooden sides and floor of the maze were painted a dull flat black The top was covered with ½" wire mesh Wire-mesh doors, hinged to the top of the maze just beyond the true-path segment of each unit, were used to limit retracing to a distance equal to the length of the true path segment of one unit, 15" Maze I was located on the floor of a relatively sound-proof room, where the preliminary runway was also placed

Maze II was identical with Maze I It was placed on a platform which could be rotated, directly above Maze I The fact that each of these two mazes was constructed of nine identical separable units made it possible to interchange any single unit with

any other unit of the maze without altering the pattern. The fact that the length of the true-path segment of each unit was the same as the length of the blind-alley segment also made it possible to rotate each unit through 180° (to interchange the true-path and blind-alley segments of each unit), again without altering the pattern of the maze. Although such variations were possible with both mazes, the changes were made only with Maze II.

An electric buzzer of low pitch and moderate intensity was placed on the floor in one corner of the room The intensity of the sound of the buzzer was not greater than that of sounds ordinarily present in the maze situation This buzzer was used to provide the animal with approximately the same opportunity to use auditory stimuli which the typical maze situation affords Five 15-watt lamps were distributed above each maze so as to illuminate various parts of the maze with fairly uniform intensity. The distribution of the lamps was the same for both mazes. No further attempt was made to eliminate shadows. The doors of the starting- and food boxes, and the doors used to limit retracing in the maze, were controlled by a string-pulley arrangement from outside the experimental room The experimenter was stationed outside the experimental room and observed the animals through a window in the door.

C. Animals and Operative Procedure The albino rats were closely inbred descendants of the Wistar experimental stock Because animals of the desired age were not available at the time needed, a small additional group of animals (those tested on Maze II) was obtained directly from the Wistar Institute. The total number of animals employed in this investigation was 157, of which 84 were male and 73 female. The average age of the animals at the time experimentation with each animal was begun was 186.4 days, with a range of 119-260 days.

The operative procedure by means of which vision and olfaction were eliminated and audition impaired was like that commonly employed in investigations in the field of the sensory control of the maze habit (1) Vision was eliminated by the removal of the entire eyeball. (2) Olfaction was eliminated by an operation in which the olfactory bulbs were severed from their attachments to the olfactory tracts. This operation involved the removal of the hair from the top of the head, a longitudinal incision in the

skin above the skull, removal of a disc of bone from the skull just above the olfactory bulbs, a transverse cut through the olfactory bulbs, removal of the olfactory tissue anterior to the cut, and several sutures in the skin which had been cut. No dressing was applied to the cut surfaces (3) Audition was impaired by means of an operation in which the skin below the external auditory meatus was slit, the tympanic membrane punctured with a probe, the system of ossicles in the middle ear disconnected from its attachments to the tympanic membrane and the inner ear, the middle ear filled with melted paraffine (which immediately solidified since the melting point was 53 C), and the cut skin sewed together. (4) An olfactory control operation was performed on one group of animals as a partial control for possible shock resulting from the olfactory operation This operation was a repetition of the olfactory operation except that the knife was not inserted into the neural tissue After the disc of bone was removed, the flaps of skin were sewed together (5) An auditory control operation was performed on one group of animals as a partial control for possible shock resulting from the auditory operation. This operation was a repetition of the auditory operation up to the point where the probe was inserted into the tympanic membrane. After the skin had been slit, the flaps were sewed together.

All operations were performed under deep ether anaesthesia. Although conditions were not entirely aseptic, precautions were taken to minimize the likelihood of infection. All instruments were sterilized in a germicidal solution, the cloth upon which the operations were performed was sterilized by heating under steam pressure, and all cut surfaces on the animal were thoroughly washed with the germicidal solution That success attended these precautions is indicated by the fact that, of a total of approximately 225 operations, only 14 cases of infection were revealed by autopsy

It is obvious that the olfactory and auditory control operations offered only partial controls for possible shock effects of the olfactory and auditory operations However, the fact that the *completed* operations had no effect upon the accuracy of maze performance showed that these controls had been unnecessary

D. Detailed Training and Retraining Procedure

1 *Experimental groups* All animals were first trained on Maze I until the criterion of mastery, 9 errorless trials out of 10

successive trials, had been reached After the learning criterion had been attained, each animal received 5 additional trials, to increase the thoroughness with which the maze was learned. (During this original training series, all animals were normal, i e, no operations had been performed upon them) During the succeeding period of seven days the animals were divided into 8 groups,[2] and each group was subjected to a different type of treatment. (1) Group 1, used as a normal control, was rested (2) Group 2 was blinded (3) Group 3 was made anosmic (4) Group 4 was made deaf. (5) Group 5 was made both blind and anosmic. (6) Group 6 was made blind, anosmic, and deaf. (7) The olfactory control operation was performed on Group 7. (8) The auditory control operation was performed on Group 8 Seven days after the last trial of the original training series, retraining of Groups 1-8 on Maze I was begun. If retention was not perfect, retraining was continued until the maze was relearned or until at least 30 retraining trials had been given (Since the maze was originally learned in an average of 33 trials, this number of retraining trials seemed sufficient.) Animals in the olfactory control and auditory control groups were discarded after they had relearned, or after they had received at least 30 retraining trials. Because all of the exteroceptive modalities with which this investigation is concerned had been eliminated from animals in the blind-anosmic-deaf group (Group 6), animals in this group were discarded after they had relearned, or after they had received at least 30 retraining trials. Since this investigation is concerned with the sensory control of the perfected maze habit, further work was done only with those animals in the remaining groups that relearned the maze

The procedure described below was now initiated in order to test the hypothesis that the absence of disturbance following the elimination of individual modalities was due to the animal's dependence upon those modalities which remained. From among the animals in the normal, blind, anosmic, deaf, and blind-anosmic groups that relearned, additional experimental groups were constituted During an interval of seven days after relearning by these groups was completed, normal animals (now called Group 9) were rested, blind animals (now called Group 10) were made anosmic, anosmic animals (now called Group 11) were made

[2]For method of selection see page 251

blind, deaf animals (now called Group 12) were made blind; and blind-anosmic animals (now called Group 13) were made deaf. At the conclusion of the period of seven days, all of the animals in Groups 9-13 were retrained a second time on Maze I. Those animals in Groups 9-12 that failed to relearn a second time within approximately 30 trials were discarded. Since all of the exteroceptive modalities with which this investigation is concerned had been already eliminated from animals in Group 13, animals in this group were discarded after they had relearned or after they had received at least 30 retraining trials From those animals in Groups 9-12 that did relearn a second time, additional groups were constituted. During an interval of seven days after the completion of the second relearning of these groups, the normal animals (now called Group 14) were rested; the blind-and-then-anosmic animals (now called Group 15) were made deaf, the anosmic-and-then-blind animals (now called Group 16) were made deaf, and the deaf-and-then-blind animals (now called Group 17) were made anosmic At the conclusion of the period of seven days, all of the animals in Groups 14-17 were retrained a third time on Maze I.

Group 18 was composed of normal animals who were originally trained on Maze I During an interval of seven days after the completion of original learning, these animals were rested At the end of this interval, retraining was begun on Maze II (from which it was believed that all constant exteroceptive stimulation was eliminated by rotation of the maze and by the interchange and rotation of its units after each trial), and continued until the maze was learned, or until at least 30 trials had been given.

The total procedure described above is summarized in Table 1 (It is to be understood that only those animals in Groups 1-17 that relearned Maze I were tested further) The purposes served by the use of each of these 18 experimental groups will be indicated in the section devoted to the interpretation of results

Nineteen animals in each of the eight basic experimental groups completed the original learning of Maze I. Of these, the results of three each in the anosmic, blind-anosmic, and olfactory control groups, and the results of two in the blind-anosmic-deaf group were discarded because autopsy revealed that an undesired effect had been produced by the operations One animal in the blind-anosmic-deaf

TABLE 1
SUMMARY OF GENERAL PROCEDURE

No normal rats in each group that completed original learning of Maze I	Treatment 7-day interval	First relearning	Treatment 7-day interval	Second relearning	Treatment 7-day interval	Third relearning
19	Rested (called Group 1)	Maze I	Rested (called Group 9)	Maze I	Rested (called Group 14)	Maze I
19	Blinded (called Group 2)	Maze I	Made anosmic (called Group 10)	Maze I	Made deaf (called Group 15)	Maze I
16	Made anosmic (called Group 3)	Maze I	Blinded (called Group 11)	Maze I	Made deaf (called Group 16)	Maze I
19	Made deaf (called Group 4)	Maze I	Blinded (called Group 12)	Maze I	Made anosmic (called Group 17)	Maze I
16	Made blind and anosmic (called Group 5)	Maze I	Made deaf (called Group 13)	Maze I		
16	Made blind, anosmic, and deaf (called Group 6)	Maze I				
16	Olf control *oper.* (called Group 7)	Maze I				
19	Aud control oper (called Group 8)	Maze I				
17	Rested (called	Maze II				

group died during the auditory operation because of an overdose of anaesthetic Thus the final numbers of animals in each of the basic groups were 19 each in the normal control, blind, deaf, and auditory control groups, and 16 each in the anosmic, blind-anosmic, blind-anosmic-deaf, and olfactory control groups. There were 17 animals in Group 18

2 *Selection of groups* Experimentation was begun with a group of 8 animals, each of the 8 basic experimental groups being represented by one animal. An animal was arbitrarily designated as a member of one of the 8 groups before experimentation was begun Whenever new animals were used, they were added in similar groups of 8 In so far as possible, a group of 8 animals was recruited from a single litter. This procedure may be regarded as essentially a series of repetitions of the entire experiment, each repetition adding one animal to each of the 8 basic groups. Any slight changes in experimental technique resulting from practice by the experimenter, or any other progressive factors present during the progress of the experiment would thus affect all of the groups equally Because of the number of intercomparisons between groups which this investigation involves, it was desirable to have the several groups as nearly alike as possible in original general ability.

3. *Training procedure* After an animal had been selected as a subject it was weighed and isolated in an individual living-cage For approximately the first half of the entire experiment this preliminary isolation period was 24 hours; for the remainder it was 48 hours The absence of food during this period of isolation served to motivate the animal Water was always available Ten trials, five on each of the following two days, were then given on the preliminary runway to accustom the animals to the general procedure to be employed during training on the maze. Just before the first trial of each day, the individual cage containing the hungry animal was transferred to one end of the preliminary runway At the other end of the runway was a similar cage containing a small pellet of milk-soaked bread. Immediately after the animal had traversed the runway and eaten the pellet, the cage containing the animal was transferred to the front end of the runway, and the living-cage which had been the starting-box was transferred to the other end for the beginning of the second trial. This procedure was duplicated during the three succeeding trials By its

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No. normal rats in each group that completed original learning of Maze I	Treatment 7-day interval	First relearning	Treatment 7-day interval	Second relearning	Treatment 7-day interval	Third relearning
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19	Blinded (called Group 2)	Maze I	Made anosmic (called Group 10)	Maze I	Made deaf (called Group 15)	Maze I
16	Made anosmic (called Group 3)	Maze I	Blinded (called Group 11)	Maze I	Made deaf (called Group 16)	Maze I
19	Made deaf (called Group 4)	Maze I	Blinded (called Group 12)	Maze I	Made anosmic (called Group 17)	Maze I
16	Made blind and anosmic (called Group 5)	Maze I	Made deaf (called Group 13)	Maze I		
16	Made blind, anosmic, and deaf (called Group 6)	Maze I				
16	Olf. control oper (called Group 7)	Maze I				
19	Aud control oper. (called Group 8)	Maze I				
17	Rested (called Group 18)	Maze II				

group died during the auditory operation because of an overdose of anaesthetic. Thus the final numbers of animals in each of the basic groups were 19 each in the normal control, blind, deaf, and auditory control groups, and 16 each in the anosmic, blind-anosmic, blind-anosmic-deaf, and olfactory control groups. There were 17 animals in Group 18.

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use, the living-cage served as starting-box during some trials and as food box during other trials. Handling of the animal was thus unnecessary at any time during training. At the end of the fifth trial the animal found a relatively large quantity of milk-soaked bread, supplemented on alternate days with bits of apple Immediately after the completion of the fifth trial the cage containing the animal was transferred to a small room adjacent to the experimental room, where the animal remained until the beginning of the next five trials, 24 hours later. Excess food was removed 25 minutes after the completion of the fifth trial Care was taken to keep the temperature of the room which housed the experimental animals, and the experimental room, at 75° F.

Twenty-four hours after the tenth trial on the runway, when, with rare exceptions, the animals ran to the food box immediately after the door of the starting-box was opened, training trials on the maze were begun. Five trials were given each day until the criterion of mastery, 9 errorless trials out of 10 successive trials, was reached Five additional trials were given, to increase the thoroughness with which the maze was mastered Entrance into a blind alley, or, after emergence from a blind alley, entrance into the true-path segment of a unit while oriented toward the starting-box (to a distance equal to the body length of the animal), was regarded as an error. The wire-mesh doors just beyond the entrance to the true-path segment of each unit (see Figure 1) were lowered immediately after the true-path segment of each unit was entered In this way retracing was limited to 15″, the length of the true-path segment of one unit.

Seven days after the last trial of the original learning series, retraining of the 8 basic groups was begun on Maze I. During this interval of seven days the 8 groups were treated in the manner described below. Twenty-four hours after the normal control animals (Group 1) had completed learning, they were weighed and returned to their individual living-cages where they remained for six more days During this period they were permitted to eat for 25 minutes each day, at the time of day when they ordinarily received their trials on the maze At the end of this period, retraining trials were begun and continued until the maze was relearned or until at least 30 trials had been given (In the retraining of all groups, retraining was continued until the maze was

relearned or until 30 trials had been given If, after 30 trials, errors were being made consistently, retraining was stopped. If errors were infrequent after 30 trials, retraining was continued for from 5 to 30 additional trials, until the maze was relearned or until the persistence of errors convinced the experimenter that the criterion would not be reached readily, at which time retraining was stopped.)

The blind, anosmic, blind-anosmic, and olfactory control groups were treated exactly as was the normal control group, except that, in addition to being weighed, the various operations were performed 24 hours after the completion of the original training series Animals in the blind-anosmic-deaf group were made blind and anosmic one day after the completion of original learning, and they were made deaf three days after the completion of original training The reasons for allowing only four days between the auditory operation and the beginning of retraining are presented below.

Tests conducted with animals upon whom the auditory operation was performed indicated that these animals were almost completely insensitive to sounds for approximately seven days after the completion of the operation. After approximately seven days, however, there was evidence of some responsiveness to sounds, although this responsiveness was slight compared with that of normal animals Because of this partial recovery during the interval of seven days, the auditory operations, whenever made, were performed three days after the completion of learning (or relearning). The first retraining trials were thus given when any recovery of auditory sensitivity was slight. Examination of the animals seven days after the auditory operation, when auditory sensitivity had increased somewhat, revealed that in many cases the paraffine with which the middle ear had been filled had disappeared. Because of the likelihood that the loss of this paraffine was responsible for the slight increase in auditory sensitivity, the middle ears of deaf animals who were blinded (Group 12) were filled with paraffine a second time, four days prior to the beginning of the second retraining series Similarly, the middle ears of deaf-and-then-blind-and-then-anosmic animals (Group 17) were filled with paraffine a third time, four days prior to the beginning of the third retraining series It is evident that the auditory operation did not completely eliminate the possibility of receiving auditory stimuli, but for convenience the

animals upon whom this operation was performed are termed "deaf."

Relatively gross autopsies were made upon animals upon whom the olfactory and the olfactory control operations were performed. Regarding the olfactory operation, three points were investigated: (1) whether the olfactory neural tissue had been only partially severed, (2) whether the frontal lobes had been cut; and (3) whether infection had developed anywhere. Positive findings on any one of these points led to the discarding of the records for the animal concerned.

Weight, as a gross measure of the general condition of health of the animal, was determined before the original training on the maze, after original learning, after each relearning, and after the last relearning.

IV. Results

The experimental results will be presented in the following order: (1) original similarity between each of the eight basic groups (Groups 1-8); (2) first relearning performances of these groups, (3) second relearning performances of Groups 9-13; (4) third relearning performances of Groups 14-17; (5) the performance of Group 18 on Maze II; and (6) a comparison between the effect, upon the accuracy of maze performance, of the elimination of individual sense modalities from normal animals and the effect of the elimination of the same modalities from defective animals

A. Original Similarity between the 8 Basic Experimental Groups. The degree of original similarity between the groups, as measured by litter identity, age, distribution of sex, and original learning scores, is indicated in Tables 2 and 3. Table 2 presents the degree to which the ideal of split-litter distribution among the several groups was approached.[8] Each group was compared with every other group, and the number of animals in any two groups who were members of the same litters was determined Thus in Table 2, "12 V" in column "N" means that 12 animals in Group V and 12 animals in Group N belonged to the same litters. ("19 V"

[8] In Table 2, and wherever the symbols appear, *N* refers to the normal control group, *V*, to the blind group, *O*, to the anosmic group, *A*, to the deaf group; *OV*, to the blind-anosmic group, *OVA*, to the blind-anosmic-deaf group; *Ocon*, to the olfactory control group, and *Acon*, to the auditory control group

TABLE 2
LITTER IDENTITY BETWEEN GROUPS
(See text for explanation)

Group →	N (19 rats)	V (19 rats)	O (16 rats)	A (19 rats)	OV (16 rats)	OVA (16 rats)	Ocon (16 rats)	Acon (19 rats)
No rats in other groups who were litter-mates of rats in group at head of column	12 V 8 O 10 A 7 OV 7 OVA 6 Ocon 11 Acon	12 N 9 O 9 A 7 OV 8 OVA 8 Ocon 11 Acon	8 N 9 V 10 A 6 OV 6 OVA 9 Ocon 10 Acon	10 N 9 V 10 O 7 OV 9 OVA 8 Ocon 9 Acon	7 N 7 V 6 O 7 A 5 OVA 7 Ocon 6 Acon	7 N 8 V 6 O 9 A 5 OV 7 Ocon 7 Acon	6 N 8 V 9 O 8 A 7 OV 7 OVA 6 Acon	11 N 11 V 10 O 9 A 6 OV 7 OVA 6 Ocon

TABLE 3
DATA ON ORIGINAL SIMILARITY BETWEEN GROUPS 1-8

Group	No	Mean age (days)	Sex		Original learning records					
					Trials		Errors		Seconds	
			Male	Female	Mean	σ_M	Mean	σ_M	Mean	σ_M
N	19	192 7	8	11	24 7	4 19	37.7	4 25	618 6	70 5
V	19	193 3	8	11	36 6	6 25	52 9	9 44	847 6	155 1
O	16	186 1	9	7	38 8	4 78	53 0	4 96	824 2	71 6
A	19	179 2	10	9	36 5	6 35	57 0	7 25	898 6	140 6
OV	16	186 1	8	8	38 9	6 21	51 8	5 75	785 6	82 8
OVA	16	185 7	7	9	29 6	4 20	40 8	4 20	669 1	66 6
Ocon	16	188 1	7	9	30 9	6 85	41 9	5 40	625.9	113 5
Acon	19	185 9	10	9	29.2	5 20	40 2	4 68	699 2	88.3

in column "N" would mean that each of the 19 animals in Group V had a litter mate in Group N) The remaining number-letter combinations in each column are to be interpreted similarly. It is evident that the ideal of complete litter identity between each of the several groups was only approximated. But these data, together with the data on age, distribution of sex, and original learning scores for each group, contained in Table 3, indicate that the several groups were originally sufficiently similar to each other so that subsequent differences in relearning performances are attributable to differences in treatment during the interval of seven days between the completion of original learning and the beginning of retraining.

B. First Relearning Performances of Groups 1-8 Table 4-8 present data on the first relearning performances of Groups 1-8 The outstanding characteristics of these results are (1) the contrast between the relearning performances of the normal, blind, anosmic, and deaf groups, on the one hand, and the relearning performances of the blind-anosmic and the blind-anosmic-deaf groups, on the other hand; and (2) the failure of some animals in certain of these groups to relearn.

Table 4 shows that all of the 19 *normal control* animals relearned, making a median of 1 2 errors and requiring a median of 1.5 trials Of the 19 *blind* animals, 17 relearned, making a median of 2 0 errors and requiring a median of 1.5 trials. The two blind animals that failed to relearn made a median of 23 0 errors during their first 30 retraining trials. Of the 16 *anosmic* animals, 14 relearned, making a median of 5 5 errors and requiring a median of 8.5 trials The two anosmic animals that failed to relearn made a median of 40 5 errors during their first 30 retraining trials Of the 19 *deaf* animals, 18 relearned, making a median of 2 5 errors and requiring a median of 2 0 trials The one deaf animal that failed to relearn made 19 errors during its first 30 retraining trials

The relearning performances of the blind-anosmic and the blind-anosmic-deaf animals offer a sharp contrast with the relearning performances of the above groups. Of the 16 *blind-anosmic* animals, only 6 relearned, making a median of 33 5 errors and requiring a median of 13.5 trials. The 10 blind-anosmic animals that failed to relearn made a median of 113 0 errors during their first 30 retraining trials Of the 16 *blind-anosmic-deaf* animals, only 6

TABLE 4
FIRST RELEARNING PERFORMANCES OF GROUPS 1-8

Group	Animals that relearned				Animals that failed to relearn			
	No	Median errors	Median trials	Median seconds	No	Per-cent-age	Median total errors first 30 trials	Median total seconds first 30 trials
N	19	1.2	1.5	16.0	0	0	0	0
V	17	2.0	1.5	46.0	2	10.5	23.0	418.5
O	14	5.5	8.5	94.5	2	12.5	40.5	1842.5
Ocon	16	1.5	1.5	17.0	0	0	0	0
A	18	2.5	2.0	31.5	1	5.3	19.0	302.0
Acon	18	1.0	1.0	10.0	1	5.3	10.0	394.0
OV	6	33.5	13.5	350.5	10	62.5	113.0	1158.0
OVA	6	26.0	19.5	446.5	10	62.5	138.0	1350.5

relearned, making a median of 26.0 errors and requiring a median of 19.5 trials. The 10 blind-anosmic-deaf animals that failed to relearn made a median of 138.0 errors during their first 30 retraining trials.

All of the animals in all of the above groups received at least 10 retraining trials. The error scores for the first 10 retraining trials of each group thus provide a *direct* comparison between the *relearning performances of all animals in each group, those that* relearned and those that failed to relearn. Table 5 presents median error scores for the first 10 retraining trials of all animals in each group.

TABLE 5
FIRST RELEARNING PERFORMANCES OF GROUPS 1-8—TOTAL ERRORS
FIRST TEN RETRAINING TRIALS
(All animals)

Group	Median	σ_{med}
N	2.42	.52
V	3.50	1.19
O	5.67	2.21
Ocon	2.75	.70
A	3.17	1.00
Acon	2.90	.56
OV	42.50	5.10
OVA	45.50	6.31

These results, like those previously presented, indicate a marked similarity between the relearning performances of normal, blind, anosmic, and deaf animals, and a marked difference between the relearning performances of these animals and the relearning performances of blind-anosmic and blind-anosmic-deaf animals. Thus, while the median error scores for the first 10 retraining trials of normal, blind, anosmic, and deaf animals were 2.4, 3.5, 5.7, and 3.2, respectively, the median error scores for the blind-anosmic and the blind-anosmic-deaf animals were 42.5 and 45.5, respectively. Table 6 (which contains critical ratios for differences between the error scores for the first 10 retraining trials of all of the 18 experimental groups) shows that the small differences between the error scores for the normal, blind, anosmic, and deaf animals were not statistically reliable, while the large differences between the error scores for these groups and the error scores for the blind-anosmic and blind-anosmic-deaf animals were highly reliable.

TABLE 6

RELIABILITY OF DIFFERENCES BETWEEN MEDIAN ERROR SCORES FOR THE FIRST 10 RETRAINING TRIALS OF GROUPS 1-18

$\left(\frac{\text{Difference}}{\text{S. D}_{diff}}\right)$

Group→ ↓	V	O→V	A→V	O	Ocon	V→O	A→V→O	A	Acon	O→V→A	V→O→A	OV→A	OV	OVA	N
N	83			1 29	38			.24	.63				7 75	6.68	
N→N		2 93	70			5 36						02			
N→N→N							1.64			1 52	69				
V		2 63	14	73				52					7 38	4 21	
O→V			2 33			31							2.15		
O					1 69	4 09	.92	1 10					6 62	5.82	
V→O							1 47						2 44		
A									.24	1 25	59	1.01	7.59	6.57	
O→V→A											53	1 44			
V→O→A												.99			
OV														31	
18	3 58			2.58				3 92					4 67	4 31	4 17

It is possible to make a *direct* comparison between the relearning performances of each group in terms of *all* of the retraining trials of *all* of the animals in each group. Since those animals that failed to relearn received at least 30 retraining trials, the number of errors made by each animal during its first (actual or hypothetical) 30 retraining trials has been determined. For those animals that relearned within less than 30 trials, a hypothetical 30-trial error score was computed as follows It was assumed that after these animals had relearned within less than 30 trials, if additional trials had been given, the average error per trial would have been the same as the average error per trial for the last 5 trials of the original learning series, *after* the original learning criterion had been reached. This was found to be .20 error per trial To determine the total number of errors for 30 retraining trials of those animals that relearned within less than 30 trials, the product of .20 and the difference between 30 and the number of retraining trials actually given was added to the number of errors made during the retraining trials actually given. For example, if a blind animal required 12 trials to relearn the maze, and made 4 errors, it was assumed that if additional trials had been given, this animal would have made an average of .20 error for each of the 18 additional trials. The hypothetical 30-trial error score for this animal would be 4 plus 18 times .20, or 7.6 errors Whenever data are presented on "errors for the first 30 retraining trials" of animals that relearned within less than 30 trials, they refer to the hypothetical 30-trial error scores of these animals. The validity of this method of computing a "30-trial error score" depends upon the validity of the assumptions that (1) the degree of skill attained after the maze was *relearned* was the same as the degree of skill attained after the maze was *originally learned*, and (2), after the same degree had been attained, accuracy of performance during succeeding trials would be the same in both cases. These assumptions seem reasonable. Since the original learning of the maze (for all of the experimental animals) required, on the average, 33 trials, an additional advantage of this method of presenting the relearning data is to provide a more direct comparison between the original and the relearning performances of each group.

Table 7 presents the median error scores for the first 30 retraining trials of each group. Again it is seen (1) that the relearning

TABLE 7
FIRST RELEARNING PERFORMANCES OF GROUPS 1-8 — TOTAL ERRORS
FIRST 30 RETRAINING TRIALS
(All animals)

Group	Median	σ_{med}
N	6.2	.76
V	7.6	2.15
O	11.7	3.45
Ocon	6.7	.80
A	7.4	1.58
Acon	6.6	.83
OV	71.5	16.46
OVA	85.0	18.27

performances of the normal, blind, anosmic, and deaf groups were markedly similar to each other, and (2) that the relearning performances of the blind-anosmic and the blind-anosmic-deaf groups were markedly inferior to the former. Thus, while the median error scores for the normal, blind, anosmic, and deaf groups were 6.2, 7.6, 11.7, and 7.4, respectively, the median error scores for the blind-anosmic and blind-anosmic-deaf groups were 71.5 and 85.0, respectively. Table 8 (which presents critical ratios for the differences between the median error scores for the first 30 retraining trials of all of the 18 experimental groups) shows that the small differences between the error scores for the normal, blind, anosmic, and deaf groups were not statistically reliable, while the large differences between the scores for these groups and the scores for the blind-anosmic and blind-anosmic-deaf groups were highly reliable.

The median error score for the first 30 retraining trials of the normal control group was 6.2, with a standard deviation of 8. Three of the 19 blind animals, 5 of the 16 anosmic animals, and 2 of the 19 deaf animals made error scores for the first 30 retraining trials which were significantly larger than the median for the normal control group. On the other hand, 15 of the 16 blind-anosmic animals, and 15 of the 16 blind-anosmic-deaf animals, made error scores for the first 30 retraining trials which were significantly larger than the median for the normal control group.

The average number of trials required by all animals for original learning of the maze was 33. The average number of errors made was 45. Of all of the normal, blind, anosmic, and deaf animals, only one anosmic animal, representing 6.2% of the anosmic animals,

TABLE 8

RELIABILITY OF DIFFERENCES BETWEEN MEDIAN ERROR SCORES FOR THE FIRST 30 RETRAINING TRIALS OF GROUPS 1-18

$\left(\frac{\text{Difference}}{\text{S D.}_{diff}}\right)$

Group→ ↓	V	O→V	A→V	O	Ocon	V→O	A→V→O	A	Acon	O→V→A	V→O→A	OV→A	OV	OVA	N
N	61			1 42	.47			.46	.40				3 94	4.26	
N→N		1 84	88			3 46						.05			
N→N→N							98			50	1.10				
V		1 78	74	89				.22					3 82	4 15	
O→V			1 37			16									
O					1 41	2.83	54	1.11					3 56	3 92	
V→O							1 64						1 82		
A									45	50	1 16	11	3 87	4 20	
O→V→A											.21	43			
V→O→A												51			
OV														.53	
18	3 67			3 18				3 78					1.48	2 00	3 90

made more than 45 errors during the first 30 retraining trials, *i. e., lost the habit completely* On the other hand, 62.5% of the blind-anosmic animals and 75 0% of the blind-anosmic-deaf animals lost the habit completely.

There was no significant difference between the relearning performance of the blind-anosmic animals and the relearning performance of the blind-anosmic-deaf animals The following facts support this conclusion (1) Of the 16 animals in *Group OV,* only 6 relearned, making a median of 33.5 errors and requiring a median of 13 5 trials The 10 animals in Group OV that failed to relearn made a median of 113 0 errors during the first 30 retraining trials. Of the 16 animals in *Group OVA,* only 6 relearned, making a median of 26 0 errors and requiring a median of 19.5 trials. The 10 animals in Group OVA that failed to relearn made a median of 138.0 errors during the first 30 retraining trials (2) The median error scores for the first 10 and the first 30 retraining trials of all of the animals in *Group OV* were 42.5 and 71 5, respectively The corresponding scores for all of the animals in *Group OVA* were 45.5 and 85.0, respectively. Tables 6 and 8 show that the differences between these scores for the two groups were not statistically reliable.

This analysis leads to the following conclusions. (1) With a few exceptions in each group, the relearning performances of the blind, the anosmic, and the deaf animals were not reliably different from the relearning performance of the normal control group. (These exceptions are the 3 blind animals, the 5 anosmic animals, and the 2 deaf animals, whose error scores for the first 30 retraining trials were significantly larger than the median of the normal control group.) (2) With a single exception in each group, the relearning performances of the blind-anosmic and the blind-anosmic-deaf animals were markedly inferior to the relearning performance of the normal control group. (These exceptions are the one blind-anosmic animal and the one blind-anosmic-deaf animal whose error scores for the first 30 retraining trials were not significantly larger than the median for the normal control group.) (3) The relearning performance of the blind-anosmic-deaf animals was not significantly inferior to the relearning performance of the blind-anosmic animals

The fact that the relearning performances of animals upon whom

the olfactory and auditory operations were performed individually (Groups O and A) were not inferior to the relearning performance of the normal control group indicates that the controls for possible shock effects from these operations (Groups Ocon and Acon) were unnecessary This conclusion is further supported by the data in Tables 4-8, where it is shown that the relearning performances of the olfactory control and auditory control groups were not reliably different from the relearning performance of the normal control group. We conclude, therefore, that, since the relearning performances of the anosmic, deaf, olfactory control, and auditory control groups were not inferior to the relearning performance of the normal control group, accuracy of maze performance was not affected by possible shock effects of the olfactory and auditory operations.

An analysis of the data on the relearning performances of those animals in Groups 1-8 that *failed* to relearn leads to the following conclusions (1) In general, the animals that failed to relearn were approximately average in the rate of original learning of the maze. (2) Most of the animals that failed to relearn made at least some progress toward relearning The most marked progress was made by the few animals in Groups V, O, A, and Acon that failed to relearn; but even in Groups OV and OVA, where the disturbance was so great, half of the animals in each group showed some progress, a few animals having almost completely eliminated errors during the last five retraining trials. It is probable that if retraining had been continued all of the animals that failed to relearn within the number of trials given would have relearned eventually (3) No particular errors can be said to have prevented final relearning, since errors were made in all of the units of the maze during the last five retraining trials.

C Second Relearning Performances of Groups 9-13 Tables 9-11 present data on the second relearning performances of Groups 9-13, whose retraining trials were begun seven days after the first relearning series had been completed[4] The outstanding character-

[4] In these tables, and wherever the symbols are used, $N \rightarrow N$ refers to normal control animals who were tested after the first relearning, $V \rightarrow O$ refers to blind animals who were made anosmic after the first relearning, $O \rightarrow V$ refers to anosmic animals who were made blind after the first relearning, $A \rightarrow V$ refers to deaf animals who were made blind after the first relearning; and $OV \rightarrow A$ refers to blind-anosmic animals who were made deaf after the first relearning

istics of these results are (1) the contrast between the relearning performances of Groups N→N, A→V, and OV→A, on the one hand, and the relearning performances of Groups O→V and V→O, on the other hand; and (2) the failure of some animals in certain of these groups to relearn.

First, let us compare the second relearning performances of the several groups. Table 9 shows that all of the 16 animals in *Group N→N* relearned, making a median of 7 errors and requiring a median of 7 trials. Of the 13 animals in *Group A→V*, 11 relearned, making a median of 2.0 errors and requiring a median of 2.0 trials. The two animals in Group A→V that failed to relearn made a median of 15.0 errors during the first 30 retraining trials. Of the 6 animals in *Group OV→A*, all relearned, making a median of 1.0 error and requiring a median of 1.0 trial.

Turning to Groups O→V and V→O, we find that their relearning performances were markedly inferior to the relearning performances of the above groups. Of the 15 animals in *Group O→V*, only 9 relearned, making a median of 4.0 errors and requiring a median of 5.0 trials. The 6 animals in *Group O→V* that failed to relearn made a median of 132.5 errors during the first 30 retraining trials. Of the 12 animals in *Group V→O*, 10 relearned, making a median of 30.5 errors and requiring a median of 12.5 trials. The two animals in Group V→O that failed to relearn made a median of 76.0 errors during the first 30 retraining trials.

To provide a *direct* comparison between the relearning performances of *all* animals in each group, those that failed to relearn as well as those that relearned, error data on the first 10 retraining trials are presented in Table 10. These data, like those previously presented, indicate a marked similarity between the relearning performances of Groups N→N, A→V, and OV→A, and a marked difference between the relearning performances of these groups and the relearning performances of Groups O→V and V→O. Thus, while the median error scores for the first 10 retraining trials of all of the animals in Groups N→N, A→V, and OV→A were 1.5, and 4.5, and 2.0, respectively, the corresponding scores for all of the animals in Groups O→V and V→O were 23.0 and 26.0, respectively. Table 6 shows that the relatively small differences between the error score for Group N→N and the error scores for Groups A→V and OV→A were not statistically reliable, while the large

TABLE 9
SECOND RELEARNING PERFORMANCES OF GROUPS 9-13

		Animals that relearned			Animals that failed to relearn			
Group	N	Median *errors*	Median *trials*	Median *seconds*	N	Per- *centage*	Median total errors first 30 *trials*	Median total seconds first 30 *trials*
N→N	16	7	7	7	0	0	0	0
A→V	11	2.0	2.0	92.0	2	15.4	15.0	363.0
OV→A	6	1.0	1.0	18.0	0	0	0	0
O→V	9	4.0	5.0	111.0	6	40.0	132.5	1117.0
V→O	10	30.5	12.5	309.0	2	16.7	76.0	943.5

TABLE 10
SECOND RELEARNING PERFORMANCES OF GROUPS 9-13 — TOTAL ERRORS
FIRST 10 RETRAINING TRIALS
(All animals)

Group	Median	σ_{med}
N→N	1.5	.75
A→V	4.5	3.50
OV→A	2.0	.58
O→V	23.0	7.31
V→O	26.0	4.45

differences between the error score for Group N→N and the error scores for Groups O→V and V→O were highly reliable.

To provide a direct comparison between the relearning performances of each group in terms of *all* of the retraining trials of *all* of the animals in each group, error scores for the first 30 retraining trials of each group are presented in Table 11. Again it is found

TABLE 11
SECOND RELEARNING PERFORMANCES OF GROUPS 9-13 — TOTAL ERRORS
FIRST 30 RETRAINING TRIALS
(All animals in each group)

Group	Median	σ_{med}
N→N	6.7	.67
A→V	13.8	7.49
OV→A	6.1	11.98
O→V	40.4	18.29
V→O	37.7	8.51

that the relearning performances of Groups A→V and OV→A were not significantly inferior to the relearning performance of Group N→N, while the error scores for Groups O→V and V→O were much larger than the error score for Group N→N. Thus, while the median error scores for the first 30 retraining trials of Groups N→N, A→V, and OV→A were 6.7, 13.8, and 6.1, respectively, the corresponding scores for Groups O→V and V→O were 40.4 and 37.7, respectively. Table 8 shows that the relatively small differences between the error score for Group N→N and the error scores for Groups A→V and OV→A were not statistically reliable. The large difference between the error scores for Groups N→N and V→O was highly reliable. The large difference between the error scores for Groups N→N and O→V was not highly reliable (the

critical ratio was 1.84), presumably because of the wide range of error scores for animals in the latter group

The median error score for the first 30 retraining trials of Group N→N was 6.7, with a standard deviation of 7. Four of the 13 animals in Group A→V and one of the 6 animals in Group OV→A made error scores for their first 30 retraining trials which were significantly larger than the median for Group N→N On the other hand, all of the 12 animals in Group V→O and 9 of the 15 animals in Group O→V made error scores for their first 30 retraining trials which were significantly larger than the median for Group N→N.

This analysis of the second relearning performances of Groups 9-13 leads to the following conclusions: (1) With a few exceptions in each group, the relearning performances of Groups A→V and OV→A were not inferior to the relearning performance of Group N→N. (These exceptions are the four animals in Group A→V and the one animal in Group OV→A whose error scores for their first 30 retraining trials were significantly larger than the median for Group N→N) (2) The relearning performances of all of the animals in Group V→O and of most of the animals in Group O→V were significantly inferior to the relearning performance of Group N→N (The exceptions in Group O→V are the six animals whose error scores for their first 30 retraining trials were not significantly larger than the median for Group N→N)

An analysis of the data on the retraining performances of those animals in Groups 9-13 that *failed* to relearn leads to the following conclusions. (1) In general, the animals that failed to relearn were approximately average in the rate of original learning (2) Most of the animals that failed to relearn, even in Groups O→V and V→O, where the disturbance was so marked, made at least some progress toward relearning It is probable that if retraining had been continued, all of the animals that failed to relearn within the number of trials given would have relearned eventually. (3) No particular errors can be said to have prevented final relearning, since errors were made in all of the units of the maze during the last 5 retraining trials.

D Third Relearning Performances of Groups 14-17. Table 12 presents data on the third relearning performances of Groups 14-17, whose retraining trials were begun seven days after the

TABLE 12

THIRD RELEARNING PERFORMANCES OF GROUPS 14–17

Group	Animals that relearned				Animals that failed to relearn			
	N	Median errors	Median trials	Median seconds	N	Per-cent-age	Median total errors first 30 trials	Median total seconds first 30 trials
N→N→N	11	.7	.7	.7	2	15.4	34.0	1192.0
V→O→A	3	2.8	4.0	71.0	4	57.1	27.0	490.0
O→V→A	7	5.8	2.0	66.0	2	22.2	77.0	526.5
A→V→O	3	5.0	5.0	153.0	1	25.0	44.0	625.0

completion of the second relearning series.[5] Although the small number of animals in each of these groups renders it impossible to make any conclusive generalizations regarding their relearning performances, the following characteristics may be noted (1) There is some indication that while the relearning performances of Groups N→N→N, O→V→A, and V→O→A were not reliably different from each other, the relearning performance of Group A→V→O was inferior to the relearning performances of the former groups. (2) Some animals in each of the four groups failed to relearn a third time

Table 12 shows that, of the 13 animals in *Group N→N→N,* 11 relearned, making a median of .7 error and requiring a median of 7 trial. Of the 7 animals in *Group V→O→A,* 3 relearned, making a median of 2.8 errors and requiring a median of 4 0 trials Of the 9 animals in Group *O→V→A,* 7 relearned, making a median of 5.8 errors and requiring a median of 2 0 trials Of the 4 animals in *Group A→V→O,* 3 relearned, making a median of 5.0 errors and requiring a median of 5.0 trials.

To provide a direct comparison between the relearning performances of *all* animals in each group, those that failed to relearn as well as those that relearned, median error scores for the first 10 retraining trials of all animals in each group have been determined. These scores for the first 10 retraining trials of Groups N→N→N, V→O→A, O→V→A, and A→V→O were 1 7, 5.5, 8 5, and 13 5, respectively. Although the differences between these scores were not statistically reliable (see Table 6), Group A→V→O made the largest number of errors

To provide a direct comparison between the relearning performances of each group in terms of *all* of the retraining trials of *all* of the animals in each group, error data on the first 30 retraining trials of all animals in each group have been computed. The median

[5]In this table, and wherever the symbols are used, *N→N→N* refers to normal control animals who were rested after original learning, after their first relearning, and after their second relearning; *V→O→A* refers to those animals who were blinded after original learning, made anosmic after their first relearning, and made deaf after their second relearning, *O→V→A* refers to those animals who were made anosmic after original learning, made blind after their first relearning, and made deaf after their second relearning; *A→V→O* refers to those animals that were made deaf after original learning, made blind after their first relearning, and made anosmic after their second relearning

error scores for the first 30 retraining trials of Groups N→N→N, V→O→A, O→V→A, and A→V→O were 6.8, 10.6, 13.4, and 17.5, respectively. Again, although the differences between these scores were not statistically reliable (see Table 8), Group A→V→O made the largest number of errors.

Although the small number of animals in each of the above groups makes a conclusive generalization impossible, the data suggest that the relearning performance of Group A→V→O was inferior to that of Group N→N→N, while the relearning performances of Groups V→O→A and O→V→A were more nearly like the relearning performance of Group N→N→N.

An analysis of the retraining data for those animals in Groups 14-17 who *failed* to relearn yielded the following (1) The animals in Groups N→N→N and V→O→A who failed to relearn were significantly inferior in the rate of original learning to those animals in the same groups who did relearn. The animals in Groups O→V→A and A→V→O who failed to relearn were approximately average in the rate of original learning. (2) The two animals in Group N→N→N made no appreciable progress toward relearning, possibly for the reason that they were markedly inferior in the rate of original learning. Only one of the four animals in Group V→O→A failed to make marked progress toward relearning. The one animal in Group A→V→O that failed to relearn made marked progress toward relearning. It is probable that all of the above animals would have relearned eventually if retraining had been continued. The two animals in Group O→V→A made no progress toward relearning. No explanation for this failure to progress is available. The increase in the number of errors of one animal, from 1 for the first 5 retraining trials to 16 for the last 5 retraining trials, may have been due to some organic defect not disclosed by the autopsy. (3) No particular errors can be said to have prevented final relearning, since errors were made in almost all of the units of the maze during the last 5 retraining trials.

E. Performance of Group 18 on Maze II. As previously indicated, Group 18 was composed of normal animals who were originally trained on Maze I. Seven days after the last trial of the original learning series, during which time the animals were rested, training was begun on Maze II. All constant exteroceptive stimulation, both from within the maze and from the environment

of the maze (with the possible exception of certain visual stimuli from the environment of the maze, to be discussed later), was eliminated during the trials on Maze II by the procedure described below.

After each trial, the platform on which Maze II rested was rotated so that the starting-box was in a new position in the experimental room, and all of the nine units of the maze were interchanged. For the first trial, the starting-box was in the east (directly above the starting-box of Maze I). The first unit was labeled "1," the second unit "2," etc The blind-alley segment of each unit was labeled "*a*" and the true-path segment of each unit was labeled "*b*" For the second trial, the starting-box was in the south, and the units of the maze were rearranged in the following order: 2, 8, 6, 3, 5, 1, 4, 9, and 7. At the same time that the units were interchanged, each unit was rotated so that the "*a*" segment of each unit (which was blind alley in the first trial) was true path, and the "*b*" segment (which was true path in the first trial) was blind alley. For the third trial, the starting-box was in the north, and an entirely different order of units was used For this trial the "*a*" segment of each unit was blind alley, and the "*b*" segment of each unit was true path. A schedule of 15 such different positions of the starting-box and orders of units, as determined by chance, was drawn up. For the odd trials (of the first 15 trials) the "*a*" segment of each unit was blind alley and the "*b*" segment was true path. For the even trials the "*b*" segment of each unit was blind alley and the "*a*" segment was true path. After the 15th trial the same schedule of changes was used again. It is obvious that because of these random changes, no particular olfactory, auditory, or cutaneous stimuli from within the maze or from the environment of the maze could have been utilized as directive cues

Although the changes described above eliminated all particular visual stimuli from within the maze as a source of sensory guidance, it remained theoretically possible that the lamps by means of which the maze was illuminated could have served as directive visual cues. It will be seen from Figure 1 that when the starting-box of the maze was in the north, west, or south these lamps occupied the same positions relative to the maze as when the starting-box was in the east. And, since the positions of these lamps relative to

Maze II were the same as the positions of the lamps which illuminated Maze I, the lamps remained as an unchanged part of the environment of the maze after the animals were transferred from Maze I to Maze II. The possibility that these lamps were utilized as directive cues on Maze II was tested by blinding some animals that learned this maze and retraining them a second time on Maze II The results secured will be given below on page 274. The learning performance of Group 18 on Maze II should indicate whether any exteroceptive modality was utilized on Maze I and should throw some light on the question of whether particular exteroceptive stimuli were utilized as *directive cues* (For a discussion of the latter point see page 288 below)

The outstanding characteristics of the performance of Group 18 on Maze II are. (1) Their learning performance was markedly inferior to the relearning performance of *Group N* on Maze I (2) Some animals in Group 18 failed to learn Maze II.

We shall first compare *Group N* with *Group 18* (1) All of the 19 animals in *Group N* relearned Maze I, making a median of 1 2 errors and requiring a median of 1.5 trials Of the 17 animals in *Group 18,* only 10 learned Maze II, making a median of 11.5 errors and requiring a median of 15.5 trials. The 7 animals in Group 18 that failed to learn Maze II made a median of 86.0 errors during their first 30 trials. (2) The median error scores for the first 10 and the first 30 retraining trials of *Group N* were 2.4 and 6 2, respectively The corresponding scores for all of the animals in *Group 18* were 14.8 and 43.0, respectively. Tables 6 and 8 show that the large differences between these scores for the two groups were statistically reliable. (3) For none of the animals in *Group N* was the number of errors during the first 30 retraining trials greater than the average number of errors made during original learning, i e., none of the animals in Group N lost the habit completely Group 18 required a median of 30 trials for original learning of Maze I, and made a median of 41 6 errors. Nine of the 17 animals in Group 18 made more than 41.6 errors during their first 30 trials on Maze II, i e , 52.9% of these animals lost the habit completely (Two of these 9 animals eventually learned Maze II, one in 51 trials, and the other in 52 trials.) (4) The median error score for the first 30 retraining trials of Group N was 6.2, with a standard deviation of .8. Only 2 of the 17

animals in Group 18 made error scores for their first 30 trials on Maze II which were *not* significantly larger than the median for Group N

This analysis leads to the conclusion that, with a few exceptions, the elimination of all constant exteroceptive stimulation (with the possible exception of certain visual stimulation, noted above) resulted in a significant disturbance of accuracy of maze performance (The exceptions are represented by the two animals in Group 18 whose error scores for their first 30 trials on Maze II were not significantly larger than the median error score for Group N.)

The possibility remains that the mere change from Maze I to the new situation of Maze II may have produced some disturbance in the performance of Group 18. A control experiment to test this possibility had been planned, but lack of time prevented its being carried out. However, the fact that the errors of animals in Group 18 persisted for so many trials indicates that the marked inferiority of this group cannot be attributed solely to the change from Maze I to Maze II.

An analysis of the retraining performances of those animals in Group 18 that *failed* to learn Maze II leads to the following conclusions. (1) Most of the animals that failed to learn Maze II were inferior, in the rate of learning Maze I, to those animals in Group 18 that did learn Maze II. (2) Four of the 7 animals that failed to learn Maze II made at least some progress toward learning. It is probable that if training had been continued all of these animals would have learned Maze II eventually.

To test the possibility that vision remained as a source of sensory guidance in the Maze-II situation (see above, pp 272-273), 5 animals in Group 18 were blinded 24 hours after they had learned Maze II Six days later, retraining trials on Maze II were begun To control for the interval of seven days between the completion of learning and the beginning of retraining, 3 animals in Group 18 were rested for seven days after they had learned Maze II and were then retrained on the same maze Data on the relearning performances of the two groups of animals are presented in Table 13. The 3 animals that were rested during the interval of seven days showed no forgetting. Of the 5 animals that were blinded, 2 (Nos. 169 and 192) showed no forgetting, 1 (No 170) was greatly disturbed, and 2 (Nos 174 and 190) were somewhat less

disturbed Animals 170, 174, and 190 failed to relearn within 30 trials.

TABLE 13

RELEARNING PERFORMANCES ON MAZE II OF ANIMALS IN GROUP 18
(Retraining trials were begun 7 days after completion of first learning of Maze II)

Subject	Normal animals Errors first 10 trials	Trials to relearn	Errors before relearning criterion was reached	Subject	Blind animals Errors first 10 trials	Trials to relearn	Errors before relearning criterion was reached
171	0	0	0	169	1	0	0
172	1	0	0	192	0	0	0
189	1	0	0	170	48	—	103*
				174	16	—	37*
				190	4	—	24*

* Animals 170, 174, and 190 failed to relearn They made 103, 37, and 24 errors, respectively, during their first 30 retraining trials.

We have already indicated above (pp 272-273) that the Maze-II experimental situation offered the possibility of the utilization of visual cues, in spite of the rotation of the maze and the interchange of its units, due to the symmetrical arrangement of the ceiling lights which illuminated the maze. Although the small number of animals in the two groups that were retrained on Maze II makes a conclusive generalization impossible, the disturbance shown by some of the animals that were blinded suggests that some of the animals in Group 18 that learned Maze II were dependent upon vision The fact that two of the five blinded animals were not disturbed indicates that some of the animals who learned Maze II were not dependent upon *any* exteroceptive stimulation.

F. Comparison between the Effect of the Elimination of Individual Sense Modalities from Normal Animals and the Effect of the Elimination of the Same Modalities from Defective Animals The purpose of the use of experimental Groups 9-17 was to determine whether animals who were not disturbed by the individual elimination of various modalities were dependent upon those modalities which were not eliminated. For example, if normal animals were not disturbed by the elimination of vision because they then used olfaction, and if normal animals were not disturbed by the

elimination of olfaction because they then used vision, the elimination of olfaction from blind animals should result in a greater disturbance than the elimination of olfaction from normal animals, and the elimination of vision from anosmic animals should result in a greater disturbance than the elimination of vision from normal animals. Accordingly, this section is devoted to a comparison between the effect of the elimination of individual modalities from normal animals and the effect of the elimination of the same modalities from defective animals.

1. The elimination of olfaction from normal animals vs. the elimination of olfaction from defective animals. The effect, upon the accuracy of maze performance, of the elimination of olfaction from blind animals was much greater than the effect of the elimination of olfaction from normal animals. The following comparisons between the first relearning performance of *Group O* and the second relearning performance of *Group V→O* support this conclusion: (1) Of the 16 animals in *Group O*, 14 relearned, making a median of 5.5 errors and requiring a median of 8.5 trials. The two animals in Group O that failed to relearn made a median of 40.5 errors during their first 30 retraining trials. Of the 12 animals in *Group V→O*, 10 relearned, making a median of 30.5 errors and requiring a median of 12.5 trials. The two animals in Group V→O that failed to relearn made a median of 76.0 errors during their first 30 retraining trials. (2) The median error scores for the first 10 and the first 30 retraining trials of all of the animals in *Group O* were 5.7 and 11.7, respectively. The corresponding scores for all of the animals in *Group V→O* were 26.0 and 37.7, respectively Tables 6 and 8 show that these large differences between the error scores for the two groups were quite reliable. (3) The median error score for the first 30 retraining trials of Group O was 11.7, with a standard deviation of 3.4. Eight of the 12 animals in Group V→O made error scores for their first 30 retraining trials which were significantly larger than the median for Group O (4) The median error score for the first 30 retraining trials of the normal control animals (Group N) was 6.2, with a standard deviation of .8. Five of the 16 animals in *Group O* and *all* of the animals in Group V→O made error scores for their first 30 retraining trials which were significantly larger than the median for Group N

These comparisons lead to the following conclusions: (1) With

some exceptions (represented by the 5 animals in Group O whose error scores for their first 30 retraining trials were significantly larger than the median for Group N), the elimination of olfaction from normal animals had *no effect* upon the accuracy of maze performance (2) Without exception, the elimination of olfaction from blind animals resulted in a significant disturbance of the accuracy of maze performance (3) With a few exceptions (represented by the four animals in Group V→O whose error scores for their first 30 retraining trials were not significantly larger than the median for Group O), the relearning performance of Group V→O was significantly inferior to the relearning performance of Group O.

Although the number of animals in Group A→V→O was too small for a completely valid comparison with other groups, there is some evidence that the maze performance following the elimination of olfaction from the deaf-and-then-blind animals was inferior to the maze performance following the elimination of olfaction from normal animals. The following facts support this conclusion: (1) The median error scores for the first 10 and the first 30 retraining trials of all of the animals in Group A→V→O were 13 5 and 17.5, respectively (Although these error scores were larger than the corresponding scores for Group O, Tables 6 and 8 show that the differences were not statistically reliable) (2) Two of the 4 animals in Group A→V→O made error scores for their first 30 retraining trials which were significantly larger than the median error score for the first 30 retraining trials of Group O

2. *The elimination of vision from normal animals vs. the elimination of vision from defective animals.* The effect, upon the accuracy of maze performance, of the elimination of vision from anosmic animals was much greater than the effect of the elimination of vision from normal animals The following comparisons between the first relearning performance of *Group V* and the second relearning performance of *Group O→V* support this conclusion: (1) Of the 19 animals in *Group V*, 17 relearned, making a median of 2 0 errors and requiring a median of 1 5 trials The two animals in Group V that failed to relearn made a median of 23 0 errors during their first 30 retraining trials Of the 15 animals in *Group O→V*, only 9 relearned, making a median of 4.0 errors and requiring a median of 5 0 trials The 6 animals in Group O→V that failed to relearn made a median of 132 5 errors during their first 30 retrain-

ing trials. (2) The median error scores for the first 10 and the first 30 retraining trials of all of the animals in *Group V* were 3.5 and 7.6, respectively. The corresponding scores for all of the animals in *Group O→V* were 23.0 and 40.4, respectively. Tables 6 and 8 show that the large differences between these scores for the two groups were not completely reliable, presumably because of the wide range of error scores for animals in Group O→V. (The critical ratio for the difference between the scores for the first 10 retraining trials was 2.63. The critical ratio for the first 30 retraining trials was 1.78.) (3) The median error score for the first 30 retraining trials of Group V was 7.6, with a standard deviation of 2.2. Nine of the 15 animals in Group O→V made error scores for their first 30 retraining trials which were significantly larger than the median for Group V. (4) Three of the 19 animals in *Group V* made error scores for their first 30 retraining trials which were significantly larger than the median error score for the first 30 retraining trials of Group N. Nine of the 15 animals in *Group O→V* made error scores for their first 30 retraining trials which were significantly larger than the median for Group N.

These comparisons lead to the following conclusions: (1) With a few exceptions (represented by the 3 animals in Group V whose error scores for their first 30 retraining trials were significantly larger than the median error score for Group N), the elimination of vision from normal animals had *no effect* upon the accuracy of maze performance. (2) With some exceptions (represented by the 6 animals in Group O→V whose error scores for their first 30 retraining trials were not significantly larger than the median error score for Group N), the elimination of vision from anosmic animals resulted in a significant disturbance of maze performance. (3) With some exceptions (represented by the 6 animals in Group O→V whose error scores for their first 30 retraining trials were not significantly larger than the median error score for Group V), the relearning performance of Group O→V was significantly inferior to the relearning performance of Group V.

There was almost no significant difference between the effect of the elimination of vision from deaf animals and the effect of the elimination of vision from normal animals. This conclusion is supported by the following comparisons between the first relearning performance of *Group V* and the second relearning performance of

Group A→V: (1) Of the 13 animals in *Group A→V*, 11 relearned, making a median of 2.0 errors and requiring a median of 2.0 trials. The two animals that failed to relearn made a median of 15.0 errors during their first 30 retraining trials (Compare with the corresponding data for Group V presented above.) (2) The median error scores for the first 10 and the first 30 retraining trials of all of the animals in *Group V* were 3.5 and 7.6, respectively. The corresponding scores for all of the animals in *Group A→V* were 4.5 and 13.8, respectively. Tables 6 and 8 show that the relatively small differences between these scores for the two groups were not statistically reliable. (3) Only 3 of the 13 animals in Group A→V made error scores for their first 30 retraining trials which were significantly larger than the median error score for the first 30 retraining trials of Group V. (4) Three of the 19 animals in *Group V* and 4 of the 13 animals in *Group A→V* made error scores for their first 30 retraining trials which were significantly larger than the median error score for the first 30 retraining trials of Group N.

3. *The elimination of audition from normal animals vs. the elimination of audition from defective animals.* There was almost no significant difference between the effect of the elimination of audition from blind-anosmic animals and the effect of the elimination of audition from normal animals. This conclusion is supported by the following comparisons between the first relearning performance of *Group A* and the second relearning performance of *Group OV→A*. (1) Of the 19 animals in *Group A*, 18 relearned, making a median of 2.5 errors and requiring a median of 2.0 trials. The one animal in Group A that failed to relearn made 19 errors during its first 30 retraining trials. Of the 6 animals in *Group OV→A*, all relearned, making a median of 1.0 error and requiring a median of 1.0 trial. (2) The median error scores for the first 10 and the first 30 retraining trials of all of the animals in *Group A* were 3.2 and 7.4, respectively. The corresponding scores for *Group OV→A* were 2.9 and 6.1, respectively. Tables 6 and 8 show that the small differences between these scores for the two groups were not statistically reliable. (3) Only one of the 6 animals in Group OV→A made an error score for its first 30 retraining trials which was significantly larger than the median error score for the first 30 retraining trials of Group A. (4) Two of the 19 animals in *Group A* and 1 of the 6 animals in *Group OV→A* made error scores for their first 30

retraining trials which were significantly larger than the median score for the first 30 retraining trials of Group N

Although the number of animals in Groups V→O→A and O→V→A is too small for a completely valid comparison with other groups, there is some evidence that, with some exceptions, the effect of the elimination of audition from blind-and-then-anosmic animals or from anosmic-and-then-blind animals was not significantly different from the effect of the elimination of audition from normal animals. This conclusion is supported by the following facts. (1) The median error scores for the first 10 and the first 30 retraining trials of *Group A* were 3.2 and 7.4, respectively. The corresponding scores for *Group V→O→A* were 5 5 and 10 6, respectively The corresponding scores for Group O→V→A were 8 5 and 13 4, respectively Tables 6 and 8 show that the relatively small differences between the corresponding scores for the three groups were not statistically reliable. (2) Three of the 7 animals in Group V→O→A, and 4 of the 9 animals in Group O→V→A, made error scores for their first 30 retraining trials which were significantly larger than the median error score for the first 30 retraining trials of Group A

4. *Comparison between the performance of Group 18 on Maze II and the relearning performance of Group OVA on Maze I* Although the magnitude of the disturbance produced by the simultaneous elimination of vision, olfaction, and audition, by means of operations upon the animal, was greater than the disturbance produced by the simultaneous elimination of all constant exteroceptive stimulation (with the possible exception of certain visual stimulation from the environment of the maze) by means of the manipulation of the animal's environment, both methods, with some exceptions, resulted in a significant disturbance of the accuracy of maze performance. (These exceptions are represented by one animal in Group OVA, and two animals in Group 18, whose error scores for their first 30 retraining trials were not significantly larger than the median error score for the first 30 retraining trials of Group N)

This conclusion is supported by the following comparisons between the first relearning performance of Group OVA on Maze I and the performance of Group 18 on Maze II· (1) Of the 16 animals in *Group OVA*, 6 relearned, making a median of 26.0 errors and requiring a median of 19 5 trials The 10 animals in Group OVA

who failed to relearn made a median of 138.0 errors during their first 30 retraining trials Of the 17 animals in *Group 18*, 10 learned Maze II, making a median of 11.5 errors and requiring a median of 15.5 trials The 7 animals in Group 18 who failed to learn Maze II made a median of 86.0 errors during their first 30 trials. (2) The median error scores for the first 10 and the first 30 retraining trials of all of the animals in Group OVA were 45.5 and 85.0, respectively. The corresponding scores for all of the animals in Group 18 were 14.8 and 43.0, respectively Tables 6 and 8 show that the differences between these scores for the two groups were statistically reliable (3) Twelve of the 16 animals in Group OVA and 9 of the 17 animals in Group 18 made more errors during their first 30 retraining trials than during original learning, i e, 75% of the animals in Group OVA and 52.9% of the animals in Group 18 lost the habit completely. (4) Only 1 of the 16 animals in *Group OVA* and only 2 of the 17 animals in *Group 18* made error scores for their first 30 retraining trials which were not significantly larger than the median error score for the first 30 retraining trials of Group N.

The fact that the relearning performance of Group OVA on Maze I was inferior to the performance of Group 18 on Maze II may have been due in part to the utilization by some animals in Group 18 of visual cues from the environment of the maze (the possibility of which was discussed on pages 272 and 274 above)

V Interpretation of Results

In the analysis of the first relearning performances of Groups 1-8 a marked difference was found between the effect of the individual elimination of vision, olfaction, and audition upon the accuracy of maze performance and the effect of the simultaneous elimination of the same modalities. With a few exceptions, the former resulted in no disturbance With a few exceptions, the latter resulted in a significant disturbance

Two alternative explanations for this difference suggest themselves. (1) The inferiority of the relearning performances of those animals who were deprived of more than one modality simultaneously was due to the *combined* shock effect of the operations by which these modalities were eliminated (although the shock resulting from an operation in which a single modality was eliminated was not great

enough to affect relearning performance). (2) Normal animals utilized more than one exteroceptive modality after the maze had been mastered. In the absence of any *one* of the three modalities (vision, olfaction, or audition), dependence upon the modalities which remained enabled the animals to perform without disturbance. But in the absence of more than one modality, the amount of stimulation upon which the animals could depend was so small that serious disturbance followed [The possibility that the inferior performance of the blind-anosmic animals was due to a reduction in the total quantity of stimulation, the "tonic theory" of Carr (4), is discussed below on page 288.] We shall first consider the adequacy of the explanation in terms of operative shock.

The only difference between the anosmic and the blind-anosmic groups, from the standpoint of operation, was the operation in which the animals were blinded. As was indicated in the section devoted to operative procedure, recovery from this operation was complete within a few hours If the shock resulting from the combined visual-olfactory operation was much greater than that resulting from the olfactory operation alone, such a difference should be reflected in a corresponding difference between the changes in weight for the two groups over the interval during which the two types of operations were performed. No such difference was found, the changes in weight for both groups (Groups O and OV) being negligible These considerations lead us to agree with Watson (24, p. 49) that "the shock effects of this operation [the visual] are so mild as to be hardly noticeable." The markedly inferior relearning performances of Groups O→V and V→O constitute a most convincing additional argument against the operative shock explanation of the inferior performance of Group OV. At least nine days intervened between the visual and olfactory operations upon the animals in Groups O→V and V→O. And yet, even when nine days intervened between the visual and olfactory operations, the elimination of *both* vision and olfaction resulted in a marked disturbance of relearning performance. And, since *this* disturbance cannot be explained in terms of a combined shock effect of the two operations, we believe that the marked disturbance following the *simultaneous* elimination of vision and olfaction is better explained in other terms And, since the relearning performance of the blind-anosmic-deaf group was not reliably different from that of the blind-anosmic

group, if operative shock was not the cause of the inferior performance of the latter group, the inferiority of the former group must be attributed to some other cause.

Turning to the alternative explanation, we may ask, "Which are the exteroceptive modalities whose *simultaneous* elimination resulted in so marked a disturbance?" The fact that the elimination of *both* vision and olfaction resulted in a marked disturbance, together with the fact that the elimination of vision or olfaction *individually* did not affect performance, suggests that (1) normal animals utilized *both* vision and olfaction, (2) normal animals were not disturbed by the elimination of vision alone because they then utilized olfaction, and (3) normal animals were not disturbed by the elimination of olfaction alone because they then utilized vision. The fact that the relearning performance of the blind-anosmic-deaf group was not significantly inferior to the relearning performance of the blind-anosmic group suggests that the blind-anosmic animals were not dependent upon audition. This fact, together with the fact that the elimination of audition from normal animals had no effect upon maze performance, suggests that audition is not an important factor in the maze situation.

On the basis of this analysis, we may tentatively conclude that, with a few exceptions in each case, (1) normal animals utilized both vision and olfaction, (2) normal animals were not disturbed by the elimination of vision alone because they then utilized olfaction, (3) normal animals were not disturbed by the elimination of olfaction alone because they then utilized vision, (4) neither normal, blind, anosmic, nor blind-anosmic animals utilized audition, so that audition is not an important factor in the maze situation, and (5), since normal animals did not utilize audition, deaf animals, like normal animals, were dependent upon both vision and olfaction The exceptions to these general conclusions are as follows: (1) A small percentage of normal animals did not utilize either vision or olfaction (represented by the 1 blind-anosmic animal and the 1 blind-anosmic-deaf animal whose error scores for their first 30 retraining trials were not significantly larger than the median error score for the first 30 retraining trials of the normal control group). (2) A small percentage of the normal animals was dependent mainly upon vision (represented by the 3 blind animals whose error scores for their first 30 retraining trials were significantly larger than the

median score for the normal control group) (3) A small percentage of the normal animals was dependent mainly upon olfaction (represented by the 5 anosmic animals whose error scores for their first 30 retraining trials were significantly larger than the median score for the normal control group). (4) A small percentage of the normal animals utilized audition (represented by the 2 deaf animals whose error scores for their first 30 retraining trials were significantly larger than the median for the normal control group). Evidence regarding the rôle of proprioception will be presented in detail later in this section.

In general, the second and third relearning performances of Groups 10-13 and 15-17 on Maze I, and the performance of Group 18 on Maze II, confirm these tentative conclusions: (1) The facts that, with some exceptions, the effect of the elimination of olfaction from blind animals was significantly greater than the effect of the elimination of olfaction from normal animals, and that the effect of the elimination of vision from anosmic animals was significantly greater than the effect of the elimination of vision from normal animals, confirm the conclusion that most normal animals utilized both vision and olfaction (and that they were not disturbed by the elimination of olfaction alone because they then utilized vision; and that they were not disturbed by the elimination of vision alone because they then utilized olfaction). (2) The fact that there was some evidence that the effect of the elimination of olfaction from deaf-and-then-blind animals was greater than the effect of the elimination of olfaction from normal animals likewise tends to confirm the conclusion that normal animals were not disturbed by the elimination of olfaction alone because they then utilized other exteroceptive modalities. (3) The fact that, with a few exceptions, the effect of the elimination of vision from deaf animals was not significantly greater than the effect of the elimination of vision from normal animals indicates that most blind animals did not utilize audition, and that most deaf animals did not utilize vision alone. (4) The fact that, with a few exceptions, the elimination of audition from blind-anosmic animals, from anosmic-and-then-blind animals, and from blind-and-then-anosmic animals was not significantly greater than the effect of the elimination of audition from normal animals confirms the conclusion that most of the animals deprived of both vision and olfaction did not utilize audition, and that audition is

not an important factor in the maze situation. (5) The fact that, with a few exceptions, the elimination of all constant exteroceptive stimulation (with the possible exception of certain visual stimulation from the environment of the maze) by means of changes in the animal's environment (Group 18) resulted in a significant disturbance is in accord with the conclusion that *some* exteroceptive modalities were utilized by most animals.

The purpose of this investigation includes an evaluation of the extent to which proprioception, as well as vision, olfaction, and audition, *is utilized in the double-alternation spatial-maze habit after* this habit has been mastered So far, our discussion has been concerned with the rôle of the exteroceptive modalities The question now arises as to what evidence our results contain concerning the extent to which proprioception is normally utilized It will be recalled that (1) the simultaneous elimination of vision and olfaction *resulted in a complete loss of the habit (i e., relearning involved* more errors than did original learning) by 62.5% of the animals; (2) the simultaneous elimination of vision, olfaction, and audition resulted in a complete loss of the habit by 75% of the animals, and (3) the elimination of almost all constant exteroceptive stimulation from the Maze-II situation resulted in a complete loss of the habit by 52 9% of the animals In other words, the elimination of exteroceptive stimulation *alone* completely abolished the habit for most animals. This suggests that in this maze only exteroceptive stimulation was normally utilized to a significant degree by most animals.

A small percentage of the animals was not disturbed at all by the simultaneous elimination of vision and olfaction, of vision, olfaction, and audition, or by the elimination of almost all constant exteroceptive stimulation (Group 18). Some animals, although disturbed by these procedures, did not lose the habit completely This suggests *that some animals were wholly, and some animals were partially,* dependent upon some non-exteroceptive process after the maze had been mastered under normal conditions. Many animals from whom vision, olfaction, and audition were eliminated, simultaneously or successively, although disturbed, were able to relearn in the absence of all of these modalities (This was true of 4 animals in Group OVA, 6 animals in Group OV→A, 3 animals in Group V→O→A, 5 animals in Group O→V→A, and 2 animals in Group A→V→O.)

Five animals in Group 18, although disturbed, were able to learn Maze II. And many animals in Groups OV and OVA who failed to relearn (half of the animals in each group) showed progress toward relearning. These facts suggest that specific exteroceptive stimulation is not *necessary* for mastery of the double-alternation maze, and that many animals utilized some non-exteroceptive process after the maze had been relearned.

This analysis leads to the following conclusions concerning the rôle of proprioception: (1) After the maze was mastered under normal conditions, (*a*) most animals were not dependent upon proprioception for errorless performance; and (*b*) a few animals were entirely, and a few animals were partially, dependent upon some non-exteroceptive process (2) Many animals who utilized exteroceptive stimulation after the maze had been mastered under normal conditions utilized some non-exteroceptive process after the maze was relearned in the absence of exteroceptive stimulation. (3) Whether the non-exteroceptive process utilized by the above animals was proprioception remains to be determined.

The maze employed in this study was of the double-alternation type The turns involved after the maze was entered were right, right, left, left, right, right, left, and left The distance between each turn was the same, viz., 15″. After the maze was entered, the animal was required to move 15″ in a forward direction and turn to the right, move 15″ in a forward direction and turn to the right, move 15″ in a forward direction and turn to the left, etc Thus, the pattern of the maze immediately preceding the second turn to the right and the first turn to the left was the same, viz , an angle of 90° to the right followed by a straightaway of 15″. According to Hunter (11), in a double-alternation maze of this type (in which the distance between each turn is the same) the proprioceptive consequences of successive movements are equated, and thus cannot serve as differential cues It seems to the writer, however, that the possibility has not been eliminated that proprioception is a possible source of sensory guidance in the double-alternation maze. It is true that, immediately preceding the second turn to the right and the first turn to the left, the *patterns of the maze* were identical, but does it necessarily follow that the *movements of the animal* through parts of the maze identical in pattern were the same? It is known that the speed with which the animal travels is greater in the part

of the maze near the food box than in the part near the starting-box.[6] (This is true even in the preliminary runway, where successive parts of the path to be traversed are completely identical.) The difference in speed with which successive parts of the maze are traversed may be due to a difference in the number of steps taken in successive parts, to a difference in the speed with which successive turns are made, etc. And this difference in speed must be accompanied by some difference in movement which could conceivably give rise to differential proprioceptive consequences. Although the presence of identical angles and linear distances preceding the second turn to the right and the first turn to the left *limits* possible differences in movements through these parts, it does not necessarily follow that these movements are identical. The possibility remains (and the difference in speed with which successive parts of the maze are traversed suggests that this is actually the case) that, even within successive parts of the maze which are identical in geometric pattern, the movements through these parts *may be different*. Since it is the *movement of the animal* rather than the geometric pattern of the maze which supplies possible proprioceptive cues, we believe that proprioception remains as a possible source of sensory guidance in the double-alternation spatial maze. (In order for proprioception to serve as a source of sensory guidance, it would not be necessary that the animal's movements in a given unit be identical in successive trials. Only a relatively constant *difference* between movements in successive identical units from trial to trial would be necessary.)

So, regarding the rôle of proprioception, we conclude that, although proprioception was not significantly employed by most of the animals who learned under normal conditions, proprioception *may* have been utilized (entirely) by those few animals who were not disturbed by the elimination of exteroceptive stimulation and by those animals who relearned in the absence of exteroceptive stimulation, and (partially) by those animals who did not lose the habit completely following the elimination of exteroceptive stimulation. The fact that most of the animals did not utilize proprioception under normal conditions is in accord with the suggestion that the double-alternation type of maze *limits* possible differences between movements (and their proprioceptive consequences) through parts of the maze which

[6]This statement is based upon the writer's gross observation of the behavior of the rat in the maze. It should be verified experimentally.

are identical in geometric pattern. The fact that many animals utilized some non-exteroceptive process (a few after original learning, many after relearning) is in accord with the suggestion that proprioception is a possible source of sensory guidance in the double-alternation maze. Whether this non-exteroceptive process actually was proprioception must be determined experimentally Hunter's theory of a symbolic process (11) and Lashley and Ball's theory of a central neural engram (15) are alternative explanations.

It has been shown that most animals were dependent upon both vision and olfaction for errorless performance in the maze after it had been mastered under normal conditions. The question of the exact nature of the rôle of these modalities will now be considered. (1) Visual and olfactory stimulation may have had a "tonic" effect upon the animals (4), and the elimination of these modalities may have resulted in a disturbance because the total quantity of stimulation, and its tonic effect, was seriously reduced. (2) Particular visual and olfactory stimuli, or patterns of stimuli, may have been utilized as directive cues, and the elimination of these cues may have been the cause of the disturbance in the accuracy of the relearning performance.

Let us first consider the adequacy of the tonic-effect theory. (1) Liggett (16) found that the speed with which anosmic animals traversed his maze was greater than that of normal animals, and concluded that his results did not support the tonic-effect theory. (2) The results of the present investigation likewise fail to support this theory. Both the relearning performance of animals from whom vision and olfaction were eliminated simultaneously by means of operation and the relearning performance of animals from whom practically all *constant* exteroceptive stimulation was eliminated (Group 18 on Maze II) showed a marked disturbance in accuracy *In the latter case there was no reduction in the quantity of stimulation* It is true that the disturbance in the latter case may have been due either to the introduction of new stimuli which may have had a distractive effect or to the elimination of particular exteroceptive stimuli or patterns of exteroceptive stimuli which were utilized as directive cues on Maze I. The fact that the disturbance on Maze II was not limited to the first few trials immediately following the introduction of the animals to the "variable" situation suggests that the inferior performance on Maze II was not due to a distractive effect of new exteroceptive stimuli.

We conclude that this analysis *suggests* that the disturbance following the elimination or constant variation of exteroceptive stimulation was due to the elimination of particular visual and olfactory stimuli, or patterns of stimuli, which served as directive cues A conclusive answer to this question requires that an experiment be conducted which has this problem as its definite objective

VI CONCLUSIONS

1 Previous investigations of the sensory control of the maze habit have shown that no single sense modality is *necessary* for the acquisition of the maze habit, but they have thrown little light upon the question of the extent to which each sense modality is *normally* utilized, either during the acquisition of the maze habit or after its acquisition

2 This investigation has shown that the *normal* sensory control of the double-alternation spatial-maze habit, after this habit has been acquired, is multiple

a Most animals utilize both vision and olfaction

b Most animals do not utilize audition

c. Non-exteroceptive processes (including proprioception) are not normally utilized by most animals

d. Some animals who utilize vision and olfaction can re-acquire the maze habit in the absence of exteroceptive stimulation.

e The non-exteroceptive process utilized by the few animals who do not normally utilize vision, olfaction, or audition, and by those animals who can re-acquire the habit in the absence of exteroceptive stimulation, remains to be identified. This non-exteroceptive process *may* be proprioception.

3. The exact nature of the rôle of vision and olfaction in this maze habit remains to be determined. Our results suggest that specific visual and olfactory stimuli, or patterns of stimuli, are utilized as directive cues.

4 The effect upon the accuracy of maze performance of the elimination of an individual sense modality is not a satisfactory measure of the extent to which that modality is normally utilized

a. If sensory control is multiple, dependence upon the modalities which remain will mask the extent to which any single eliminated modality is normally utilized.

b In the double-alternation spatial maze, *both* vision and

olfaction are normally utilized. Animals from whom vision is eliminated are not disturbed because they then utilize olfaction Animals from whom olfaction is eliminated are not disturbed because they then utilize vision.

5 Whether the normal sensory control of the type of maze ordinarily employed in animal experimentation is like that of the double-alternation maze remains to be determined.

a The double-alternation spatial maze is constructed so as to limit the possibility of the use of proprioception as a source of sensory guidance In the type of maze ordinarily employed in animal experimentation, no attempt is made to limit the possibility of the use of proprioception In such a maze, proprioception may play a much more important rôle

b The fact that the double-alternation spatial-maze habit is controlled by multiple stimuli suggests very strongly that the normal sensory control of other types of mazes is also multiple

6 *The multiple sensory control of the double-alternation spatial-maze habit is compatible with a conditioned-response explanation of the development of this habit*

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Clark University
Worcester, Massachusetts

LE CONTRÔLE SENSORIEL NORMAL DE L'HABITUDE PERFECTIONÉE DU RAT BLANC DANS LE LABYRINTHE SPATIAL À ALTERNATION DOUBLE

(Résume)

Le but de cette étude a été une évaluation du degré auquel le rat blanc dépend *normalement* de la vision, de l'olfaction, de l'ouïe, et de la proprioception pour une exécution sans erreurs dans le labyrinthe spatial à alternation double apres qu'il l'a bien appris Après que des animaux normaux avaient appris un labyrinthe de 9 parties, on a éliminé diverses modalités extéroceptives, seules et combinées, et ensuite on a teste les animaux dans le labyrinthe Chez les animaux qui avaient répeté l'apprentissage, on a

élimine d'autres modalités et on les a encore testés On a obtenu les résultats généraux suivants. (1) L'elimination de la vision, de l'olfaction, ou de l'ouie, *individuellement*, n'a pas dérangé l'execution dans le labyrinthe (2) L'élimination *simultanée* de la vision et de l'olfaction au moyen d'opérations sur l'animal, et l'elimination de *toute* stimulation extéroceptive constante au moyen de la manipulation du milieu de l'animal ont cause un grand dérangement, la plupart des animaux perdant complètement l'habitude (3) L'élimination de la vision chez les animaux anosmiques, et l'élimination de l'olfaction chez les animaux aveugles, ont cause un grand dérangement (4) L'élimination de l'ouie chez les animaux aveugles-anosmiques n'a pas cause un dérangement Ces résultats amènent la conclusion que le contrôle sensoriel normal de l'habitude perfectionnée du labyrinthe spatial à alternation double est multiple La plupart des animaux utilisent la vision et l'olfaction, et n'utilisent ni l'ouie ni la proprioception Les animaux ne sont derangés ni par l'élimination de la vision seule, ni par celle de l'olfaction seule, parce qu'ils utilisent puis l'olfaction ou la vision, respectivement L'absence d'un effet suivant l'élimination d'une seule modalité ne témoigne pas d'une façon satisfaisante que cette modalité n'est pas utilisée quand elle est disponible.

CASPER

DIE NORMALE SENSORISCHE BEHERRSCHUNG DER FERTIGEN GEWÖHNUNG WEISSER RATTEN IM DOPPELT ALTERNIERENDEN RAUMLICHEN LABYRINTH

(Referat)

Der Zweck dieser Untersuchung war eine Abschatzung des Grades, zu welchem die Albinoratte *normal* vom Sehvermogen, Geruch, Gehor, und von der Propriocеption fur fehlerlose Leistung im doppelt alternierenden raumlichen Labyrinth abhangig ist, nachdem die Beherrschung gewohnen worden ist Nachdem normale Tiere ein Labyrinth von neun Einheiten erlernt hatten, wurden verschiedene exteroceptive Modalitaten allein und in Verbindung eliminiert, und dann wurden die Tiere im Labyrinth gepruft Von diesen Tieren, die wiedererlernt hatten, wurden weitere Modalitäten eliminiert, und die Tiere wurden wiedergepruft Man erhielt die folgenden allgemeinen Ergebnisse 1. Die Eliminierung des Sehvermogens, des Geruchs oder des Gehors, je fur sich allein, hatte keine Storung der Labyrinthleistung zur Folge 2 Die *gleichzeitige* Elimination des Sehvermogens und des Geruchs durch Operationen am Tiere und die Elimination aller konstantin Reizung durch die Manipulation der Umgebung des Tieres, hatte eine auffallige Storung zur Folge Die meisten der Tiere verloren die Gewohnheit ganz 3 Die Elimination des Sehvermogens bei anosmischen Tieren, und des Geruchs bei blinden Tieren, hatte eine auffallige Storung zur Folge. 4 Die Elimination des Gehors bei blinden-anosmischen Tieren hatte keine Storung zur Folge. Diese Ergebnisse führen zum Schluss, dass die normale sensorische Beherrschung der facher Natur ist Die meisten Tiere benutzen sowohl das Sehvermogen als auch den Geruch und benutzen das Gehor oder die Proprioception nicht Die Tiere werden durch die Elimination des Sehvermogens allein oder des Geruchs allein nicht gestort, weil sie dann den Geruch oder beziehungsweise das Sehvermogen benutzen Die Abwesenheit einer Wirkung nach der Eliminierung einer einzelnen Modalitat, ist kein genugender Beweis, dass diese Modalitat nicht benutzt wird, wenn sie benutzbar ist

CASPER

EMOTIONAL STABILITY OF THE HARD OF HEARING*

From Teachers College, Columbia University

RUDOLF PINTNER

Deafness in any degree is supposed to cause emotional disturbances which make the deaf individual less stable than the hearing individual. Numerous writers, scientific and non-scientific, have made various statements with reference to such characteristics of the deaf and hard of hearing[1] The argument generally is that the loss, or partial loss, of hearing causes a disturbance of the personality in some form or other. The shock to the personality is presumed to be particularly severe if the loss of hearing comes during or after adolescence.

All such beliefs have, up to the present time, been based upon casual observations No one has attempted a strict comparison of hypacousic and hearing individuals by means of modern tests. The one notable exception is the recent work of Welles (1), who used the Bernreuter Personality Inventory with a population of about two hundred hypacousic individuals of various types, together with suitable control groups of hearing individuals. Welles found the hypacousic group to be significantly more emotional, more introverted, and less dominant than the hearing control group. He found no significant difference between the two groups on the Bernreuter measure of self-sufficiency. A small group of prominent hypacousic individuals showed no differences in these four measures as compared with hearing controls. Furthermore, the same author found no relationship between amount of neurotic tendency and such factors as number of years deafened, age at loss of hearing, residual hearing in the better ear, hours of lip-reading training, and the like

The present study carries on the work of Welles by the use of the same tests and technique with a somewhat different group of hypacousic individuals. The individuals tested by Welles con-

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[1]See the summary of such comments in Welles (1)

sisted of individuals living in large urban centers, who were members of the local societies for hard-of-hearing individuals. The individuals in this study were isolated hard-of-hearing people scattered all over the United States and Canada. Through the splendid co-operation of the editors of *The Auditory Outlook*,[2] the writer was able to obtain the names and addresses of individuals belonging to a correspondence club of isolated hard-of-hearing people To these individuals were sent a letter explaining the purpose of the investigation, a copy of the Bernreuter Personality Inventory, and a questionnaire calling for personal data with reference to deafness and social standing. In addition, a similar duplicate set of test blanks was enclosed for the hearing control individual. As in the Welles study, the hypacousic individual was asked to choose his hearing control As the letter put it.

> "Will you ask a hearing friend of yours, either as a contribution to a scientific study or as a personal favor to you, to fill out these folders as you have done and mail them in the addressed and stamped envelope? This friend should be without any hearing difficulty, of the same sex as yourself, and of about the same intelligence and social position"

The anonimity of both subject and control was carefully guarded.

Replies Received The tests and papers described above were sent to 221 hypacousic individuals scattered all over the United States and Canada Replies from 142 hypacousic subjects were received. This is a return of 64%, an extremely good return considering the amount of time and work demanded by the questionnaire. However, not all of these 142 hypacousic subjects were accompanied by control hearing individuals A total of 94 pairs (one hypacousic and one hearing control chosen by the hypacousic subject) were received, this is 43% of the 221 cases This is an extremely satisfactory return considering that the subjects had to find a hearing friend who would also be willing to fill out the questionnaire. There were 48 hypacousic questionnaires without hearing controls and 4 hearing questionnaires without hypacousic controls. In this study only the 94 cases with adequate hearing controls have been used

In the letter sent out to each subject the experimenter promised a personal report some three months later, provided the subject sent

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[2]The writer wishes to thank Mrs Laura Stovel, Associate Editor of *The Auditory Outlook*, for her help and interest in this study

his number to the experimenter. It was thus optional with the subject whether he cared to have the results of the Bernreuter test or not. To get these results he had to preserve a blank with his number for three months, then he had to mail this to the writer, paying the postage himself. Out of the 94 hypacousic individuals considered in this study 53, or 56%, took all the trouble described above in order to get the test results, of the 94 hearing controls only 29, or 31%, wrote for replies. In general, the results show a great interest on the part of the hard of hearing in an inquiry concerning their handicap, and a willingness to cooperate at the expense of time and trouble.

Similarity of Experimental and Control Groups. Whether the 221 hypacousic individuals to whom this test was sent are representative of hypacousic individuals in Canada and the United States, we do not know, whether the 142 subjects who answered the questionnaire are representative of the 221 to whom it was sent, we do not know. And it is idle to speculate. The 94 hypacousic individuals dealt with in this study do, however, seem to be representative of the 142 who sent in returns. The general distribution of N scores on the Bernreuter is not changed by the omission of the 48 cases for whom we have no hearing controls. The mid-score changes only by 2 or 3 points, which is negligible on the extensive Bernreuter scale.

More important for this study, dealing with 94 paired hearing and hypacousic individuals, is the question as to whether the method of

TABLE 1
COMPARISON OF HYPACOUSIC AND HEARING GROUPS

	Mean		Sigma			
	Exp	Cont	Exp	Cont	r	No. pairs
Age	46.15	42.80	13.60	13.65	65	77
Self—education	13.05	13.17	3.01	2.81	46	69
Self—business	11.20	12.20	1.99	2.42	54	30
Self—salary	1.38	1.84	69	86	13	28
Relative—business	11.93	11.70	2.81	2.92	31	49
Relative—salary	2.12	2.93	84	1.50	11	21
Relative—education	13.07	13.00	3.33	3.56	06	44

securing controls gave pairs similar in age, education, and social background. The pertinent facts gathered from the questionnaire are shown in Table 1. The correlations in the sixth column are between the individuals of each paired group These correlations show a fair degree of similarity between the control and experimental individual with reference to age, to the education and business of the reporter, and to the business of their nearest self-supporting relative. The rest of the table shows the means and sigmas of the two groups A study of this shows that the two groups are very similar. The groups of hypacousic and hearing individuals are only three and one-half years apart in age; they have had practically the same amount of education, showing an average of one year in college; on the Barr rating of their occupations they differ only by one point, the hearing being superior; the difference in salary is less than one thousand dollars, the control group being slightly higher; the last three rows referring to the nearest self-supporting relative are again very similar The salary items are given in thousands of dollars and we note that they are the least frequently answered. On the whole, however, we are justified in concluding that the two groups are very similar in age, education, and general social status, so far as the latter can be inferred from the information about the business of the reporter and his nearest self-supporting relative, the salaries and educational level attained. Although the correlation between the pairs for several items is at times very low, the means and sigmas show that the groups as a whole are fairly similar. It would seem, therefore, that the method of having the experimental subject pick his own control may result in fairly similar groups under favorable circumstances.

The Personality Measures. If we now compare the hypacousic and hearing groups on the four traits of the Bernreuter Inventory, we have the results shown in Table 2. The means for the two groups come first, followed by the sigmas, then the difference between the means with its sigma and the usual ratio, then the correlation between the paired individuals and the number of pairs.

The first row, B1-N, is the measure of neurotic tendency. The difference of 34 points between the means is statistically significant The hard of hearing would seem to be more emotionally unstable than the hearing The control group's mean score is equivalent to the 53rd percentile norm for Bernreuter's adult women The

TABLE 2
COMPARISON OF HYPACOUSIC AND HEARING GROUPS ON BERNREUTER INVENTORY

Trait	Mean Exp	Mean Cont	σ Exp.	σ Cont	M diff	σ diff	Ratio	r	N pairs
B1-N	13 60	—20 60	84.80	76 60	34 20	11 12	3 08	.11	94
B2-S	20 40	13 80	60 20	56 80	6 60	7 41	89	25	94
B3-I	12 40	— 7 80	52 40	45 40	20 20	7 00	2 89	04	94
B4-D	—15 40	9 80	69 00	61 00	25 20	8 98	2 81	13	94

members of this group are evidently very similar to Bernreuter's group, and, thus, our method of choosing controls seems to have led to a normal group on this trait The hypacousic group, however, obtain a mean score of 14 points, which is equivalent to Bernreuter's 69th percentile They are evidently very much above the norm in the direction of emotional instability Of the hypacousic, 64, or 68%, exceed the median of their hearing controls The difference between the hearing and hypacousic in neurotic tendency as measured by this questionnaire seems to be clearly marked

The second row shows the B2-S scores or measures of self-sufficiency. High scores indicate a preference for being alone, a tendency to disregard the advice, sympathy, encouragement of others. Low scores indicate the opposite tendency In this trait we find no difference between the hearing and the hypacousic The means and sigmas are practically identical The mean for the hypacousic group corresponds to Bernreuter's 62nd percentile norm, the mean for the hearing corresponds to the 57th percentile norm Both our groups would, therefore, appear to be slightly more self-sufficient than Bernreuter's standard group.

The B3-I scores in the third row of Table 2 give a measure on an introversion-extroversion scale. High scores show a tendency to be introverted; low scores, to be extroverted The ratio shows a fair degree of statistical reliability. The hypacousic are more introverted than the hearing. In comparison with Bernreuter's norms, the hypacousic mean corresponds to the 74th percentile, while the hearing mean corresponds to the 56th percentile Both groups are thus more introverted than Bernreuter's group, the hypacousic group very markedly so. These findings for introversion-extroversion agree with the results on neurotic tendency. As has been frequently pointed out, B1-N and B3-I scores probably measure much the same thing.

The last row of the table shows the scores for dominance. Those scoring high, in Bernreuter's words, "tend to dominate others in face-to-face situations. Those scoring low tend to be submissive." The difference between the means for our two groups is fairly reliable. The hypacousic group rate themselves as much more submissive than the hearing control group Our control group score of 10 corresponds to the Bernreuter 46th percentile, being therefore very similar to his adult women Our hypacousic group has a mean

score which corresponds to Bernreuter's 30th percentile, and thus is considerably more submissive than the average hearing individual

To sum up this comparison of the hypacousic and hearing, we may say that this hypacousic group is markedly more neurotic, more introverted, and more submissive than its control hearing group; it is, however, just as self-sufficient as the hearing control group. If we compare our hypacousic group with Bernreuter's standards for adult women, we find percentiles of 69 for neurotic tendency, 62 for self-sufficiency; 74 for introversion, and 30 for dominance This confirms the comparison of hypacousic and hearing groups in our study, except for the trait of self-sufficiency. In general, however, the picture here presented of the hypacousic individual, namely, that he is likely to be somewhat more neurotic, slightly more introverted and less dominant, seems to correspond well to ordinary observation The differences between the hypacousic and hearing are not large, but they probably are real.

Personal Factors Certain items of information about the hypacousic individuals were gathered by means of the questionnaire It will be of interest to note whether any of these items are significant with reference to the B1-N or neurotic-tendency measure. This measure is chosen because it differentiates most significantly between the hypacousic and hearing groups The correlation ratios between neurotic tendency and three of these personal items are as follows:

	eta yx	eta xy	Mean	N
Age of loss of hearing	.08	01	21.6	78
Number of years deafened	09	02	25 8	78
Hours of lip-reading instruction	03	02	78 6	35

It might have been supposed that the age at which hearing became affected would have influenced the individual's adjustment to life, the earlier the age, the more readily might the individual adjust But we find no evidence for this in our data Neither do we find that the greater number of years of deafness plays any measurable part in adjustment, as might also have been supposed. It was thought also that the individual who had studied lip-reading for a long time might be better adjusted, but here also our correlation is zero Note, however, that the number answering this item is very small

There is no relationship therefore between neurotic tendency and

TABLE 3
COMPARISON OF BI-N SCORES FOR THREE PERSONAL FACTORS

	Yes			No					
	M	σ	N	M	σ	N	M diff	σ diff	Ratio
Training in lip-reading	14.40	76.80	46	40.00	82.40	32	25.60	18.45	1.39
Use of hearing-aid	29.40	75.20	35	20.40	83.00	44	9.00	17.84	50
Tinnitus	30.40	80.20	59	14.20	82.40	19	16.20	21.60	75

any of these three factors The mean age of loss of hearing is 21.6 years; the mean number of years of deafness is 25.8; and the mean estimated number of hours of lip-reading instruction is 78 6 hours

Three other personal items may best be studied by comparing the difference between the mean scores in neurotic tendency of those answering "yes" and of those answering "no." Table 3 presents the data The first row of the table tells us that 46 subjects said they had training in lip-reading, whereas 32 said they had none. The untrained group has a higher neurotic score by 25.6 points, although the difference between the means is not statistically reliable. With a larger and more representative population of hypacousic individuals, this difference might become significant, and, if so, the hope would arise that training in lip-reading might help toward personal adjustment. From the second row in the table we learn that 35 subjects used mechanical aids for hearing, whereas 44 did not, and we find no difference in neurotic score between these two groups The last row tells us that 59 individuals suffered from tinnitus and 19 did not The neurotic score of the sufferers is 16 points higher than that of the non-sufferers It shows more emotional instability, although the difference is not statistically reliable. Welles found a much larger difference in the same direction as our data. If we combine the cases reported by Welles with our own, we have the following:

	Yes			No					
	M	σ	N	M	σ	N	Diff	σdiff.	Ratio
Tinnitus	+7 3	86 0	187	—22 5	85 8	83	29 8	11 24	2 65

Here the difference between the two groups is about 30 points and the ratio of the difference to its standard deviation is almost three. It would seem that persistent head noises are likely to go along with a higher neurotic score.

Interest in Results As explained above, those who cared to have individual scores could get them by writing to the experimenter after a three months' interval. We have seen that 56% of the hypacousic and 31% of the hearing controls took the trouble to write for results, thus indicating their great interest in this experiment Are the more neurotic or the less neurotic individuals primarily interested in knowing their own scores? Again we might theorize at length about this point, but let us look at the results The distributions of B1-N scores for those asking for results and those not

TABLE 4
COMPARISON OF VARIOUS HYPACOUSIC GROUPS

	N	B1-N Mean	B1-N Hearing percentile	B2-S Mean	B2-S Hearing percentile	B3-I Mean	B3-I Hearing percentile	B4-D Mean	B4-D Hearing percentile
Successful (Welles)	31	−62.7	36	+48.8	79	−21.5	46	+57.2	73
Welles's total	196	−13.1	57	+17.1	60	+ 2.3	66	+11.1	47
Urban (Welles)	46	− 0.1	64	+10.4	56	+ 6.9	69	− 0.1	40
Total hypacousic	290	− 4.7	66	+18.1	61	+ 5.3	68	+ 2.5	41
Isolated	94	+13.6	69	+20.4	62	+12.4	74	−15.4	30

asking are practically identical both for the hypacousic and for the hearing. So similar are they that we have not calculated means and sigmas and differences. For the hypacousic the mid-scores are +18 for those asking for results and +26 for those not asking; for the hearing controls the mid-scores are —30 and —32 respectively. Hence we may conclude that interest in knowing about oneself does not seem to be related to neurotic tendency.

Various Hypacousic Groups. By incorporating the different hypacousic groups studied by Welles with our group, we may compare various groups of hard-of-hearing individuals. The mean scores on the four traits are shown in Table 4, together with the equivalent percentiles for hearing adults taken from Bernreuter's latest norms. The first group, called "successful," consists of 31 prominent hard-of-hearing individuals tested by Welles, the second group is the total group tested by Welles, which includes the successful and the urban group. The urban group is the carefully paired group of Welles, consisting of members of leagues for the hard-of-hearing in urban centers. The total hypacousic group contains all cases tested by Welles and the writer up to the present time. The last group consists of the isolated rural individuals discussed in this study.

The groups have been arranged in order of increasing neurotic tendency. The equivalent hearing percentile shows that the successful hypacousic are less neurotic than ordinary hearing individuals.[3] All the other groups are more neurotic than the hearing. The total hypacousic group is at the 66th percentile for the hearing. The isolated hypacousic show the highest neurotic score. Reasons for this may be either that they have gravitated to more isolated regions because they are fundamentally neurotic, or the isolation of their lives has increased their neurotic tendency.

On the B2-S or self-sufficiency score all the groups are above the hearing median. All the hypacousic groups are much alike, except the successful group. The hypacousic differ only slightly, if at all, from the hearing on this trait.

The measure of introversion or B3-I shows results very much

[3]The equivalent hearing percentiles are taken from Bernreuter's norms for adult women. Some of the hypacousic groups contain a few men, but there are so few that they would not materially affect the general results. The Welles total group consists of women only, among the isolated group there are only twelve records from men.

like those for neurotic tendency It is probably much the same thing The successful group falls below the hearing median, and it is again sharply differentiated from the other deaf groups The isolated are very introverted, the mean approximating the 74th percentile for the hearing.

The measure of dominance or B4-D shows the successful group very dominant and again sharply differentiated from the other deaf groups. All the other deaf groups are less dominant than the average hearing group. The isolated group scores are very low on this trait. As a group they lack self-confidence and are more heavily weighted with feelings of inferiority than is usual among hearing people.

All of the comparisons made between the various hypacousic groups show the successful group at one end and the isolated at the other. Those who have become recognized as successful among their hard-of-hearing comrades are characterized as stable, dominant, extroverted, and very self-sufficient. On the other hand, the isolated hypacousic are unstable, introverted, and submissive The hypacousic as a group are also characterized as unstable, introverted, and submissive, but not to the same extent as the isolated group

If the traits measured by the Bernreuter are native traits, then we may say that those in the successful group became successful because they possessed more of such traits. If the traits are purely due to environmental opportunities, then they developed such traits owing to a greater number of opportunities. Undoubtedly, neither of these extreme statements is true. The successful were probably better endowed to begin with and also had better opportunities The isolated are probably less well endowed to begin with and lack many opportunities to learn how to make adequate emotional adjustments The fact, however, that the hypacousic as a group fall decidedly lower on three of the traits than do their hearing controls, who in other respects are much like them, points to the loss of hearing as being a major factor in the poorer adjustment of the hypacousic When one considers the mental shock resulting from the loss of hearing, one is surprised that the hypacousic do as well as we have seen they do on these questionnaires That so many reach or exceed the median score for the hearing leads one to believe that careful training and guidance may help many more hypacousic individuals to attain adequate emotional adjustment

Personal History One page of the questionnaire was reserved

for a personal opinion as to how well adjusted the subject felt he had been before his loss of hearing; as to how he felt at the time he realized that he was becoming deaf; and how well adjusted he thought he was at the present time Most of the subjects wrote something on this page. Some of them covered several additional sheets and gave a more or less detailed personal history. Most of them seemed amazingly frank in their statements and the writer felt, when reading these statements, that many of the subjects welcomed this opportunity to write about themselves, either because it was a relief to them to tell their troubles or because they hoped it might be of service to other hypacousic individuals.

These personal statements were roughly tabulated in the categories shown below:

Before hearing loss		At present time	
Well-adjusted	30	Well-adjusted	23
Fairly well-adjusted	8	Reconciled	42
Not well-adjusted	14	Not well-adjusted	17
No statement	39	No statement	9

Before the loss of hearing a great many felt they had been well-adjusted, happy individuals. Undoubtedly this period would seem a "golden age" to them as they look at it from their present viewpoint, as hard-of-hearing individuals The surprising thing is that as many as fourteen could be classified as maladjusted during this period. Many made no real statement about their feelings. Some indicated that defective hearing had been present so early in life that they never had known a period free from this handicap

Looking now at the classification of the group at the present time, we note at once that almost all of them could be classified, only nine giving no hint at all as to their present adjustment The largest group is the "reconciled" group. Here are the individuals who have recovered from their initial shock and are making the best of a handicap that cannot be cured, they would like things to be different, but, since that cannot be, they do the best they can Perhaps the majority of people, hearing as well as hypacousic, would belong to this group. Turning now to the well-adjusted group, one is surprised to find so many who state that they are happy and contented with life These are the optimists They have reconstructed their lives as hard-of-hearing individuals. A few even feel that their handicap has made them face reality, struggle with it, and conquer it in a way

they never would have done, had they not gone through the terrible experience of losing their hearing. Their struggle has brought forth personal resources which otherwise might have remained undiscovered. Then we have the group of individuals who at present do not feel well adjusted to life They describe themselves as unhappy, nervous, hopeless, or depressed. Some have a feeling of resentment against a fate that has treated them so scurvily. Why should they be picked out to suffer this terrible affliction? In several cases, however, one feels, as one reads their frank statements about their difficulties, that loss of hearing has little to do with their present maladjustment; that this is due primarily to family or economic worries, which would be just as acute even if they were normal hearing individuals But, of course, all of them feel that they could do better if they had their hearing, and so they feel that the chief cause of their maladjustment is their hearing handicap.

If, now, we inquire as to the feelings of our subjects at the time of their loss of hearing we have the following classification.

Despair, resentment, etc	31
Lost nerve	4
Humiliation, shame	10
Gradually withdrew	7
Never really unhappy	8
No real statement	31

Despair and resentment, either or both, are the most common descriptions of the feelings of these individuals The realization that they will never again be able to hear normally comes as a profound shock, particularly when the loss of hearing is sudden and when it comes during adolescence or later A feeling of humiliation and shame is frequently mentioned, particularly when the loss comes during adolescence, and this feeling of shame or humiliation is often caused or intensified by the behavior of hearing individuals in contact with the subject. The hearing person is impatient, thinks the deaf man is inattentive, that he can hear if he wants to, and so on

Some of the shame, humiliation, and despair could be assuaged by educating the hearing about the problems of the hard-of-hearing and the deaf.

The large number of cases where we have no real statement of feeling at the time of beginning deafness is due to the fact that loss of hearing in many cases begins early in life and progresses so gradually that there is no point of time where hearing ends and hypacou-

sia begins In these cases shock is avoided The individual merges slowly into the hard-of-hearing group

In these personal histories we noted in ten cases where lip-reading was specifically mentioned as the chief means of adjustment, although no question as to lip-reading had been asked in this part of the questionnaire The important part played by lip-reading in personal adjustment was felt so strongly in these ten cases that it was especially emphasized

Personal Histories and Neurotic Inventory The personal history contained the subject's account of his adjustment at the present time. These accounts were grouped by the writer into three categories: (1) well-adjusted, (2) reconciled; (3) maladjusted. Many statements were so indefinite that they could not be classified Several subjects gave no real statement as to their present adjustment. The results from 77 women and 11 men were available. It will be of interest to compare these groups with their scores on the Bernreuter for neurotic tendency The mean scores for the women are as follows:

	Mean	Hearing percentile	N
Well-adjusted	+ 7 8	67	23
Reconciled	+14 1	69	43
Maladjusted	+47 7	81	11

The means increase in the right direction The maladjusted group shows marked neurotic tendency and is definitely set off from the other two groups. The mean score is equivalent to the 81st percentile for hearing women The maladjusted show their deficiency quite definitely in answering the Bernreuter Inventory The other two groups are not really differentiated by means of the Bernreuter Inventory. There is little difference between the means (although it is in the right direction), and the overlapping of scores is very great We find in both groups individuals making very high neurotic scores A subject's statement may seem to indicate splendid adjustment to life, or it may seem to indicate that the subject is reconciled to her handicap of deafness, and, nevertheless, the score on the Bernreuter may show a very high degree of neurotic tendency Considering, however, the subjective nature of the individual's statement and the unreliability of the writer's classification, we may conclude that on the whole this comparison strengthens our belief in the validity of the Bernreuter Inventory

The results for the men are limited to eleven papers, too few for any statistical evaluation. Four were classified as well adjusted and only one of these had a high neurotic score as compared with hearing percentiles. Four were classified as maladjusted and three of these had high neurotic scores. Of the three in the "reconciled" group, one rated high, one medium, and one low in neurotic tendency.

Happiness and Neurotic Tendency One item on the questionnaire was, "Are you happy in your present employment?" Of the 94 individuals considered in this study, only 30 answered by an unqualified "Yes," and only 11 by an unqualified "No" All the rest modified their statements in various ways or left this question unanswered. Taking those who answer "Yes" or "No," we have the following means for neurotic tendency on the Bernreuter·

	Mean	Hearing percentile	N
Yes	+24 9	73	30
No	+22.3	72	11

Here the means for the two groups are practically identical. Those who consider themselves happy in their present employment and those who do not are not different in their neurotic tendency scores The mean score of those who answer this question by either "Yes" or "No" is somewhat above the mean neurotic score for our total group, which is +13.6 The difference is not very great. Are the neurotic more likely to answer in an unqualified manner? Are they more likely to go to extremes in their feelings and statements?

Summary. This study reports the results of the Bernreuter Inventory for 94 hypacousic individuals living in small towns and rural communities They are compared with hearing individuals chosen by themselves The hypacousic and hearing groups are very similar in age, education, and social background The results of the Bernreuter test show our hypacousic group to be decidedly more neurotic, more introverted, and more submissive than its control hearing group. If we compare our hypacousic group with Bernreuter's standards for women in general, we find that they deviate in the above three traits and, in addition, show themselves more self-sufficient than hearing women.

We find no relationship between neurotic tendency and age of loss of hearing, number of years deafened, number of hours of lip-reading instruction, or use of a mechanical hearing-aid Our data do, however, suggest that those who are free from head noises and those who

have had training in lip-reading are likely to have slightly lower neurotic scores than those who suffer from tinnitus and than those who have had no training in lip-reading.

A comparison of our isolated hypacousic group with Welles's urban and successful groups shows the isolated group to be more heavily loaded with the undesirable traits. This was to be expected. Their isolation restricts the opportunities for readjustment and re-education.

A study of the personal histories of our hypacousic group gives us added confidence in the Bernreuter Inventory. Those who are clearly maladjusted show a very high neurotic score There is, however, only a slight difference in neurotic score between the well-adjusted and the intermediate or reconciled group.

The picture of the average hypacousic individual given us by the Bernreuter test does not show such a marked deviation from the average hearing individual as might have been imagined from *a priori* considerations alone. The shock of deafness is great, particularly if it comes suddenly and in adolescence That so many individuals would seem to be able to readjust their lives more or less satisfactorily with little or no help, would lead one to hope that a deeper understanding of the peculiar psychological difficulties involved might eventually give rise to a technique of re-education that would bring about an adequate readjustment more speedily and to a larger number of individuals

Reference

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Teachers College
Columbia University
New York City

LA STABILITÉ ÉMOTIVE DE CEUX QUI ONT L'OREILLE DURE

(Résumé)

Cette étude mesure la stabilité émotive des adultes qui ont l'oreille dure Des individus isolés à l'oreille dure sont comparés avec des amis comparables mais normals. On a employe l'Inventaire de Personnalité de Bernreuter, un questionnaire, et une histoire personnelle.

L'Inventaire montre que la personne à l'oreille dure est décidémment plus névrosique, plus introverti et plus soumis que celle qui entend bien On n'a trouve aucune relation entre la tendance névrosique et l'âge auquel l'ouie a été perdue, les années pendant lesquelles on a souffert la surdite, les heures d'instruction pour lire les lèvres ou l'emploi d'une aide mecanique pour mieux entendre Les résultats suggèrent que ceux qui ne souffrent pas le tintement et ceux qui ont appris à lire les lèvres auront probablement des résultats névrosiques moins élevés Une comparaison de ce groupe isolé et des groupes urbains montre que ce groupe possède beaucoup plus de traits non désirables Les histoires personnelles montrent que ceux qui sont clairement mal adaptés ont un résultat nevrosique très élevé

Le portrait de l'individu à l'oreille dure sur l'Inventaire ne montre pas que déviation si marquée de celui qui entend bien qu'on l'aurait pu croire Le choc de la surdité est grand, surtout si elle arrive tout à coup et pendant l'adolescence Il paraît que plusieurs individus savent rajuster leur vie d'une manière plus ou moins satisfaisante avec très peu d'aide ou sans aide Une compréhension plus profonde des difficultés psychologiques spéciales ici en jeu pourrait faire naître à la longue une technique d'une instruction nouvelle qui ferait venir une nouvelle adaptation satisfaisante plus rapidement et à plus grand nombre d'individus

PINTNER

DAS AFFEKTIVE GLEICHGEWICHT DER SCHWERHÖRIGEN

(Referat)

In dieser Untersuchung wurde das affektive Gleichgewicht schwerhoriger Erwachsener gemessen Isolierte schwerhorige Individuen wurden mit ahnlichen, normal horenden Freunden verglichen Es wurden zu der Untersuchung das Bernreuter'sche Personlichkeitsinventar [Bernreuter Personal Inventory], ein Fragebogen, und Selbstbiographien [personal history] der Versuchspersonen verwendet

Durch das Bernreuter'sche Inventar wird darauf hingewiesen, dass die Schwerhörigen bestimmt starker zur Nervositat neigen [more neurotic], als die normal Horenden, starker nach Einwarts gekehrt sind [more introverted], und leichter nachgeben [more submissive] Man fand keine Beziehung zwischen der Neigung zur Nervositat einerseits und dem Alter der Versuchsperson zur Zeit der Verlust des Gehors, Zahl der im Lesen von den Lippen [lip reading] verbrachten Stunden, oder Gebrauch eines mechanischen Horapparates anderseits Die Befunde weisen darauf hin, dass diejenigen Versuchspersonen die frei von Tinnitus waren und diejenigen, die im Lesen von Lippen geubt waren, in Bezug auf Nervositat die niedrigsten Zahlen lieferten Durch eine Vergleichung dieser Gruppe isolierter Individuen mit stadtischen Gruppen wird erwiesen, dass erstere [?] [them] viel schwerer mit nicht-wunschenswerten Eigenschaften beladen ist. Die Selbstbiographien zeigen, dass diejenigen Versuchspersonen die

sich dem Leben bestimmt *nicht* gut angepasst haben [are clearly maladjusted] eine sehr hohe Zahl nevropathischer Eigenschaften aufweisen [have a very high neurotic score].

Das durch das Bernreuter'sche Inventar gegebene Bild des schwerhörigen Individuums weicht von dem Bild des normal Horenden nicht so stark ab, wie man es vielleicht erwartet hatte Die Erschutterung des Taubweidens [the shock of deafness] ist eine starke besonders wenn sie plotzlich und in der Jugend kommt Vielen Individuen, aber, scheint es moglich zu sein, sich mehr-minder gut, mit wenig oder keiner Hilfe von Aussen, dem Leben anzupassen Ein tieferes Verstandnis fur die besonderen Schwierigkeiten der Schwerhorigen wurde vielleicht schliesslich eine Technik der Neuerziehung erzeugen welche schneller und fur eine grossere Zahl der Individuen eine gute Neu-Anpassung [readjustment] herbeifuhren wurde

PINTNER

TWIN-SIMILARITIES IN PERSONALITY TRAITS*[1]

From the Psychological Laboratories of Stanford University

HAROLD D. CARTER

It is desirable that quantitative studies be made in order to give more exact and scientific information on the popularly accepted view of similarity of twins in personality traits The importance of extending studies of twin-similarities to traits other than intelligence is obvious, but the movement has been held in check by the slow development of measuring instruments. It seems likely that such study will not only contribute much-needed information concerning twins, and the nature-nurture question, but will also further the aims of those interested in the measurement of personality traits. Knowledge of twin-similarities, bearing as it does upon the origin and development of the traits investigated, should contribute much to our understanding of the traits, and hence facilitate the construction of suitable instruments for measurement

In dealing with personality trait constellations, there is likelihood that some facts will be discovered which do not parallel exactly those which are found for intelligence measures. For example, the writer has made the clinical observation that unlike-sex twins are in some ways more similar than fraternal like-sex twins, and it is likely that quantitative studies in this field will provide more accurate descriptions of that fact. Perhaps personality traits are more affected by training and experience; if so, statistical studies will undoubtedly show results of interest for comparison with facts already established by use of intelligence tests.

THE DATA

The Bernreuter Personality Inventory was administered to a group of 133 pairs of twins, or a total of 266 subjects. This group included both like- and unlike-sex pairs, and among the subjects were

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[1]The study reported in this paper was carried out as part of a more intensive investigation of twins which the writer conducted at Stanford University as Social Science Research Council Fellow Grateful acknowledgment is made to Dr Lewis M Terman for helpful criticisms and suggestions

15 pairs of mature individuals, varying in age from 20 to 65 years. The others represent a random selection from the junior- and senior-high-school populations of San Jose, Palo Alto, and San Francisco. Separate treatment has been given the results for these disparate groups. The subjects were classified into monozygotic and dizygotic groups, using the techniques described by Bonnevie (3), Dahlberg (4), Siemens (8), Newman (7), Muller (6), and others, and now quite generally accepted.

The Bernreuter Personality Inventory affords measures of four traits Table 1 presents some information concerning these traits,

TABLE 1
RELIABILITY COEFFICIENTS* AND INTERCORRELATIONS FOR THE TRAITS MEASURED BY THE BERNREUTER PERSONALITY INVENTORY
(Data obtained on 128 Stanford students)

	B1-N	B2-S	B3-I	B4-D
B1-N (neurotic tendencies)	88	—.39	93	—82
B2-S (self-sufficiency)		85	—28	50
B3-I (introversion)			85	—73
B4-D (dominance)				88

*The reliability coefficients were obtained by the split-half technique, and corrected by the Spearman-Brown formula

obtained from the manual (1) For further information concerning the development and use of the test, the reader should consult the work of Bernreuter (2) As evidence for the validity of the measures, the manual presents the correlations with other measures which had already been developed for the separate traits. These coefficients are very high.

The use of this test offers special advantages, in that it is easily administered, subjects enjoy taking it, and a minimum amount of time on the part of the subjects is required These features, and the elimination of objectionable items, favor more adequate cooperation on the part of the subjects, which is quite essential because the data are based on self-estimates. In this study very good cooperation was secured, no coercion was used, and none was necessary. Testing was done individually, and in small groups, and was preceded by a few preliminary explanations designed to promote cooperation Antagonistic responses were not aroused, the subjects took the tests willingly and with an eagerness to find out the results, which were subsequently reported to them

The blanks were scored for all four measures, although it is recog-

nized that B1-N and B3-I are rather highly correlated Means and standard deviations of raw scores were computed for males and females separately, and standard scores computed Thus sex differences were eliminated, making it possible to deal with the combined groups in computing coefficients of resemblance As the distributions of scores seemed to be approximately normal, this procedure was preferred rather than use of the percentile scores to eliminate sex differences.

Influences of Age and Intelligence

Although it happens that the group is relatively homogeneous with respect to age, in view of the fact that age changes in some traits are very rapid at this stage of development, it was considered desirable to study the possible influence of this factor on the scores obtained Table 2 shows the age correlations.

TABLE 2

Showing the Correlations of Age with Sigma Scores on the Four Scales of the Bernreuter Personality Inventory, for a Group of 231 Junior- and Senior-High-School Students*

The ages of the subjects ranged from 12 to 19 years inclusive, with a mean of 16 1 years and a standard deviation of 1 48 years

	N	*r*	P E
CA and B1-N	231	00	044
CA and B2-S	231	09	044
CA and B3-I	231	04	044
CA and B4-D	231	15	043

*In the group were 107 girls and 124 boys Since sigma scores were obtained separately for boys and girls, sex differences in means and standard deviations have been eliminated before combining the groups.

Consideration of the actual distributions suggests that the correlations in Table 2 are not low primarily because of the curtailment of range, but rather because there is in fact a very low correlation of these functions with age. Scores in a vocabulary or arithmetic test would have shown a much higher correlation over the same age range. The main fact to which attention is called is that the coefficients are sufficiently small to show that the contribution of age to individual differences in the traits here dealt with is negligible for this group of subjects Partialling out age would not appreciably lower the coefficients of resemblance reported later in this paper. Care has been taken to accord separate treatment to the small group of older subjects, in order that any age factor possibly operative over a wider range cannot influence the results.

For a considerable number of the subjects, measurements of intelligence were available in the school records. Table 3 presents the correlations of these intelligence scores with the traits of personality

TABLE 3

SHOWING THE CORRELATIONS OF TERMAN GROUP TEST IQ'S WITH SIGMA SCORES ON THE FOUR SCALES OF THE PERSONALITY INVENTORY, FOR A GROUP OF 148 JUNIOR- AND SENIOR-HIGH-SCHOOL STUDENTS*

	N	*r*	P E
IQ and B1-N	148	04	055
IQ and B2-S	148	10	054
IQ and B3-I	148	05	055
IQ and B4-D	148	— 01	055

*This group for which intelligence measures were available included 81 boys and 67 girls This sample is, so far as is known, fairly representative of the total group The IQ's ranged from 73 to 140, with a mean of 102.7 and a standard deviation of 13.8 points The ages ranged from 12 to 19 inclusive, with a mean of 15.9 and a standard deviation of 1.57 years The correlation between IQ and age was — 19 for this group of 148 cases

For measurement of the relationship between these variables and intelligence, the present data are imperfect, but still of considerable value. They show, for a relatively unselected group of school subjects, that almost no correlation is to be expected between these variables and IQ The absence of significant correlation means that the coefficients of resemblance reported later are not due to dependence of the traits in question upon general intelligence.

Measurements of Resemblance

Separate treatment has been given to a group of 15 pairs of mature identical twins. Table 4 shows the results obtained for the various groups of identical twins only The results of Table 4 suggest that the degree of similarity found for identical twins is very much the

TABLE 4

SHOWING THE CORRELATIONS OF SCORES ON THE FOUR SCALES OF THE PERSONALITY INVENTORY, FOR THE SUBGROUPS OF IDENTICAL TWINS, AND FOR THE COMBINED GROUPS*

	Correlation coefficients			
	B1-N	B2-S	B3-I	B4-D
School group, 40 pairs	61	59	43	75
Mature group, 15 pairs	69	13	57	67
Combined group, 55 pairs	63	44	50	71

*The single noteworthy discrepancy in the values for the separate groups is in the trait B2-S, where the correlation for mature identical twins is unduly low Omitting one pair, the coefficient for the remaining group of 14 mature pairs becomes 49 and the correlation for the combined group becomes 53 Hence the discrepancy seems to be caused largely by the grossly different scores of a single pair

same for the youthful group and for the older group, and no serious error would result from use of the coefficients obtained on the combined group as the most representative figures for the resemblance of monozygotic twins. However, to make the figures strictly comparable, the correlations obtained on the school group alone are used

TABLE 5
SHOWING THE CORRELATIONS OF SCORES ON THE FOUR SCALES OF THE PERSONALITY INVENTORY, FOR MONOZYGOTIC TWINS, FRATERNAL LIKE-SEX TWINS, AND UNLIKE-SEX TWINS
These are comparable groups of junior- and senior-high-school students

	No of pairs	Raw r	PE	Corrected coefficients* 4	5	6
BI-N (neurotic tendencies)						
Identical twins	40	61	.07	55	.69	.62
Fraternal like-sex twins	43	32	09	32	36	36
Unlike-sex twins, double-entry	35	.18	11	.30	20	34
Unlike-sex twins, single-entry	35	19	11		22	
B2-S (self-sufficiency)						
Identical twins	40	59	.07	.58	69	68
Fraternal like-sex twins	43	—14	.10	—24	—16	—28
Unlike-sex twins, double-entry	35	12	11	08	.14	09
Unlike-sex twins, single-entry	35	.13	11		15	
B3-I (introversion)						
Identical twins	40	43	09	40	50	.47
Fraternal like-sex twins	43	40	09	42	47	49
Unlike-sex twins, double-entry	35	.18	11	27	21	.32
Unlike-sex twins, single-entry	35	.18	11		.21	
B4-D (dominance)						
Identical twins	40	.75	05	76	85	86
Fraternal like-sex twins	43	34	.09	33	39	.38
Unlike-sex twins, double-entry	35	18	.11	22	20	25
Unlike-sex twins, single-entry	35	19	11		22	

*Column 4 gives the correlations corrected for range only, Column 5 gives the coefficients corrected for attenuation only, and Column 6 presents the results of correction for both range and attenuation The correction for range was done in order to make these several groups more comparable. The ranges for the separate groups were corrected to equal that for the total group For discussion of these statistical procedures, see Kelley (5)

All correlations were calculated by a double-entry system, but those for unlike-sex pairs were checked by use of the single-entry system also, with the male's score on one axis and the female's score on the other The agreement is, of course, very good

in Table 5, for comparison with the results obtained for fraternal twins. The results in Table 5 show that the identical twins are in

general most similar, unlike-sex twins least similar (except in self-sufficiency), and fraternal like-sex twins intermediate. The exceptions are, however, very important In neurotic tendencies, identical twins are significantly more similar than the fraternal like-sex or unlike-sex groups. In introversion it appears that like-sex pairs are considerably more similar than unlike-sex pairs, but the monozygotic twins are not significantly more similar than the dizygotic like-sex pairs. In dominance, the greater similarity of identical twins is more marked than in any of the other traits.

Perhaps the most interesting peculiar finding is that for self-sufficiency. There the like-sex fraternal twins are dissimilar, although not significantly so. The obtained correlation is negative—a finding which is very rare in studies of family resemblances The scatter-diagrams for these correlations are presented as Tables 6, 7, and 8. It is the aim of the writer to collect additional data, to increase the size of the groups and reduce the probable errors of the correlations, as a further attack upon the problem.

TABLE 6

SCATTER-DIAGRAM SHOWING THE CORRELATION OF SCORES ON THE SELF-SUFFICIENCY SCALE, FOR 43 PAIRS OF FRATERNAL LIKE-SEX TWINS

A double-entry system was used in the plotting. The plotted points represent the sigma scores of the twins on the self-sufficiency scale The number in each cell is the number of plotted points which fell in that cell The correlation coefficient is —14 and its probable error is 10

74-76 9																		
71-							1											
68-														1				
65-					1		1	1	1	1								
62-			1					1		1					1			
59-			1							1								
56-					1		3	1	1	2		2						
53-					1		2		1	1	2							
50-							1	3			1	2	1	1	1			
47-					1	3	1	1			1	1			1			
44-					1		1	4	1	3		1		1	1			
41-						1		1	1	1	2	3			1		1	
38-							1		3									
35-								1	1		1	1				1		
32-			1															
29-				1									1	1				
26-																		
23-25 9																		
	23-25 9	26-	29-	32-	35-	38-	41-	44-	47-	50-	53-	56-	59-	62-	65-	68-	71-	74-76.9

TABLE 7

SCATTER-DIAGRAM SHOWING THE CORRELATION OF SCORES ON THE SELF-SUFFICIENCY SCALE, FOR 40 PAIRS OF IDENTICAL TWINS

A double-entry system was used in the plotting. The plotted points represent the sigma scores of the twins on the self-sufficiency scale. The number in each cell is the number of plotted points which fell in that cell. The correlation coefficient is 59 and its probable error is 07.

	23-25.9	26-	29-	32-	35-	38-	41-	44-	47-	50-	53-	56-	59-	62-	65-	68-	71-	74-	77-79.9
77-79.9																			
74-														1					
71-												1							
68-											2						1		
65-										1									
62-										2	1							1	
59-											1	1							
56-							1		1		2	2	1				1		
53-						1	1		2	1		2	1	1		2			
50-			1						1	2	1			2	1				
47-					2			2		1	2	1							
44-				2			1	4	2										
41-					1	3		1			1	1							
38-					1	2	3				1								
35-			1			1	1		2										
32-								2											
29-					1					1									
26-																			
23-25.9																			

While speculation at this point may be premature, it may nevertheless be pointed out that the results concerning self-sufficiency are quantitative data in support of the writer's point of view gained from intensive study and close personal observation of twins He has observed that because of attitudes toward each other the fraternal like-sex twins are often less closely associated in some respects than the unlike-sex pairs The interests of like-sex fraternal twins are considerably different A condition of rivalry exists between them, and in many instances there is an absence of that attitude of comradeship which is present to a considerable degree among unlike-sex pairs, and to a very marked degree among identical twins Perhaps self-sufficiency in one tends to cause dependence (i.e , a lack of self-sufficiency) on the part of the other

TABLE 8

SCATTER-DIAGRAM SHOWING THE CORRELATION OF SCORES ON THE SELF-SUFFICIENCY SCALE, FOR 35 PAIRS OF UNLIKE-SEX TWINS

A double-entry system was used in the plotting. The plotted points represent the sigma scores of the twins on the self-sufficiency scale. The number in each cell is the number of plotted points which fell in that cell. The correlation coefficient is .12 and its probable error is .11

	23-25.9	26-	29-	32-	35-	38-	41-	44-	47-	50-	53-	56-	59-	62-	65-	68-	71-	74-	77-	80-82.9
80-82.9												1								
77-																				
74-																				
71-									1			1								
68-																				
65-				1								1								
62-						1	1		1	1	1	1								
59-													2							
56-											1			1	1		1			1
53-			1			1	1			1		1		1						
50-							1	1	2		1			1						
47-		1	1		2		2	3	2	2				1			1			
44-					1	1	1		3	1										
41-							2	1	2	1	1			1						
38-								1			1			1						
35-								1	2											
32-															1					
29-									1		1									
26-									1											
23-25.9																				

SUMMARY, CONCLUSIONS, AND SUGGESTIONS

The Bernreuter Personality Inventory was applied to 133 pairs of twins, classified as to type. The four traits measured by the test have been shown to be practically independent of age influences and of intelligence as measured by the Terman Group Test. The greater similarity of monozygotic twins is evident to a marked degree in the measures of dominance, neurotic tendencies, and self-sufficiency, but not in introversion. Like-sex pairs were definitely more similar than unlike-sex pairs in introversion, and to a lesser extent in neurotic tendencies and in dominance. An extremely interesting new problem has been indicated by the finding of a negative correlation between like-sex fraternal twins in self-sufficiency. Although this particular finding is not demonstrated to be significant, it is sufficiently important to merit further investigation. The measurements of the personality trait constellations were made by use of a standard test, with scales of known reliability and validity.

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Institute of Child Welfare
University of California
Berkeley, California

LES RESSEMBLANCES DE TRAITS DE PERSONNALITÉ CHEZ LES JUMEAUX

(Résumé)

Cent trente-trois paires de jumeaux, dont 55 monozygotes, 43 fraternels de même sexe, et 35 paires non de même sexe, ont subi l'Inventaire de Personnalité de Bernreuter. Entre les membres des paires de jumeaux identiques, les corrélations brutes sont de 0,61 pour les Tendances névrosiques, de 0,59 pour la Suffisance, de 0,43 pour l'Introversion et de 0,75 pour la Dominance Pour les jumeaux fraternels de même sexe les coefficients correspondants sont de 0,32, de —0,14, de 0,40, et de 0,34 respectivement Pour les jumeaux non de même sexe ils sont de 0,18, de 0,12, de 0,18, et de 0,18 respectivement. L'âge et l'intelligence n'influent pas sur ces corrélations.

La plus grande ressemblance des jumeaux monozygotes se montre à un degré marqué dans les mesures de la Dominance, des Tendances névrosiques, et de la Suffisance, mais ne se montre pas dans celle de l'Introversion Les paires de jumeaux dizygotes de même sexe sont definiment plus semblables que les paires non de même sexe dans l'Introversion, et d'une façon moins marquée dans les Tendances névrosiques et la Dominance Dans la Suffisance, c'est le contraire Un nouveau problème intéressant s'est montré quand l'on a trouvé une corrélation négative entre les jumeaux fraternels de même sexe dans la Suffisance.

La correction des corrélations pour l'atténuation, et la comparaison des résultats pour les jumeaux monozygotes et les jumeaux dizygotes montrent que pour ces traits de personnalité les coefficients de ressemblance

sont moins élevés que ceux ordinairement rapportés pour les mesures de l'intelligence et du rendement On croit que ceci est caractéristique des traits qui sont influencés plus facilement par les influences du milieu

CARTER

ÄHNLICHKEITEN IN BEZUG AUF PERSÖNLICHKEITS-EIGENSCHAFTEN BEI ZWILLINGEN

(Referat)

Das Bernreuter'sche Persönlichkeitsinventarium [Bernreuter Personality Inventory] wurde auf 133 Zwillingspaare angewendet, einschliesslich 55 monozygotische (eineiige) Zwillingspaare, 43 geschwisterliche Zwillingspaare (nicht eineiige) des selben Geschlechtes [like-sex fraternal], und 35 Zwillingspaare ungleichen Geschlechtes In Bezug auf Mitglieder identischer Zwillingspaare betrugen die Rohkorrelationen bezüglich der Neigungen zur Nervosität [neurotic tendencies] 61, bezüglich des Selbstvertrauens 59, bezüglich der Introversion 43, und bezüglich des Dominierens [dominance] 75 Die entsprechenden, an geschwisterlichen Zwillingen gleichen Geschlechtes ermittelten Korrelationskoeffizienten betrugen respektiv 18, .12, 18, und 18. Diese Korrelationszahlen werden durch Alter und Intelligenz nicht beeinflusst

Die stärkere Ähnlichkeit der monozygotischen Zwillinge zeigt sich in hohem Grade in den Messungen des Dominierens der Neigungen zur Nervosität, und des Selbstvertrauens, nicht aber in Bezug auf die Introversion Gleichgeschlechtliche zweieiige [dizygotic] Zwillingspaare weisen eine bedeutend stärkere Ähnlichkeit auf, als Paare ungleichen Geschlechtes, in Bezug auf Introversion und, in geringerem Grade, in Bezug auf Neigungen zur Nervosität und auf das Dominieren. In Bezug auf Selbstvertrauen ist das Verhältnis das entgegengesetzte Durch die Entdeckung einer negativen Korrelation zwischen geschwisterlichen Zwillingen gleichen Geschlechtes in Bezug auf das Dominieren wird auf eine interessante neue Aufgabe hingewiesen.

Unter Berücksichtigung der vermindernden Einwirkungen auf die Korrelationszahlen [correcting the correlations for attenuation] wurden die an monozygotischen mit den an zweieiigen Zwillingen erhaltenen Befunden verglichen Man fand, dass diese, an Eigenschaften der Persönlichkeit ermittelten Ähnlichkeitskoeffizienten [coefficients of resemblance] niedriger sind, als die gewöhnlich für Messungen der Intelligenz und der Leistungsfähigkeit [achievement] gemeldeten. Der Verfasser ist der Meinung, dass dieser Befund für Eigenschaften, die durch äussere Einwirkungen leichter beeinflusst werden, typisch ist

CARTER

THE SLEEP OF YOUNG CHILDREN*

From the Institute of Euthenics, Vassar College

MARTHA MAY REYNOLDS AND HELENA MALLAY

In recent years, interest in the sleep of the preschool child has centered around two questions How much sleep should the young child have and how could he be enticed to take it The literature abounds with dogmatic statements of how much the two-, three-, and four-year-olds should sleep each day and gives valuable suggestions for getting them to do this. But in spite of the earnest efforts of parents and teachers, the practice of many children falls far short of the standards. What is the cause of this state of affairs? Are the standards too high or the adults unable somehow to provide adequate conditions for the children to attain them?

It was in order to find partial answers to these questions that this study of sleep was undertaken at the nursery school of the Institute of Euthenics at Vassar College during the summers of 1931 and 1932.

Before describing the conditions under which the data were gathered, it will be profitable to review some of the available sources of information about the sleep of preschool children

The largest and oldest class of material is composed of the opinions of experts. It is not surprising to find disagreement among the authorities since almost no experimental evidence was used as the basis for these opinions. A difference of five hours in the estimated amount which a two-year-old child should sleep is found. Obviously, something more than expert opinion is needed to answer the questions raised at the beginning of this article.

The second source of material is composed of experimental studies and therefore offers more valuable evidence. Most of the data has been gathered by means of parents' records, however, and there has been increasing dissatisfaction with this method The reasons for this are obvious: Parents are not scientifically trained and they are very busy people, so that it is questionable whether their records are sufficiently reliable to be the basis for scientific studies of the amount

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of children's sleep However, two studies of this type have been selected for comparison of their results with those of the present study These were "A Study of Sleeping Habits of Children" by Chant and Blatz (1) and "The Sleep of Young Children" by Foster, Goodenough, and Anderson (2).

Nursery schools have conducted experimental studies of children's habits of sleeping during the afternoon nap period at school. In methods of gathering data, these studies are distinctly superior to those which use parents' records, but, since they deal with a small portion of the child's total sleep, they offer insufficient evidence for the solution of the bigger problems.

There is need then for studies made under scientific supervision of the night as well as the day sleep It is easy to see why these studies have not been made in great numbers It is too difficult to find the children under the necessary conditions Orphanages or camps for small children are among the few sources of material and these offer obvious drawbacks A study of the day and night sleep in a group of young orphanage children has recently been completed by Mary A. Wagner (3) at the University of Iowa The authors of the present study wish to express their appreciation of Miss Wagner's courtesy in permitting them to view the results before publication

The nursery school of the Vassar Institute of Euthenics presented then an unusual opportunity for the study of the problem of the sleep of young children For twenty-four hours a day, seven days a week, for six weeks, the children were under the care of experienced nursery-school teachers, and everything possible was done to provide optimum conditions for their growth The combination of so-called expert management of the children and scientific recording over such a long period of time is to be found in very few places which care for young children.

Method Used in the Present Study

During the summer schools of 1931 and 1932, the data for the present study were gathered No controlled experiment was set up The records kept were "all in the day's work" as they are in the usual nursery school. Each teacher was responsible for noting when a child went to bed, when he went to sleep, and when he awakened The associate director of the nursery school, Gertrude

Porter Driscoll, assisted in planning the records and was responsible throughout the six weeks for the general atmosphere of the life in the dormitory. One of the authors of this paper, Miss Mallay, checked the records daily and, although no special conditions were set up as controls, every effort was made to encourage accurate record keeping. The members of the staff were informed of the plans for the study and the daily and weekly results were frequently brought to their attention.

The question may properly be raised whether the records kept by the teachers were sufficiently accurate to justify their use as a basis for a scientific study of sleep. Several reasons have led the authors to believe that they were.

In the first place, recording the time a child goes to sleep is a part of the training of every nursery-school teacher. She is taught to observe the child closely and to recognize the objective signs of the onset of sleep. It can therefore be assumed that the nursery-school teachers at the Institute of Euthenics nursery school knew *how* to record the necessary data concerning the children's sleep.

Secondly, in this particular situation, conditions for accurate recording were facilitated by having two or three teachers on duty with ten children at bedtime and by providing small pads on the doors of the children's rooms on which the immediate records were noted. It was easy, therefore, to record the time a child was put to bed and the time he went to sleep, since there were so few children to a teacher.

A further justification for the assumption that the teachers' records were worthy of serious consideration is to be found in a study made by Wagner (3) in which teachers' data concerning children's sleep were checked by an expert recorder. It was found that "the percentage of agreement, within a five-minute interval, as based on 194 child observations was as follows: child asleep 97.42, child awake 98.93."

It is therefore probable that the data gathered at the Vassar College nursery school of the Institute of Euthenics were sufficiently reliable to serve as the basis for a study of the sleep of young children.

It should further be noted that this study is merely quantitative and that no effort was made to include data of a more qualitative nature. The amount of sleep which the child took was the only fac-

tor studied. Depth of sleep, amount of restlessness, and other things may be more important aspects than the amount of sleep a child takes, but, since these could not be measured objectively, they were not included in this study.

Conditions under Which Study Was Made

Before taking up the statistical treatment of the results, it will be profitable to mention briefly the general conditions of the Institute of Euthenics during the summers of 1931 and 1932. One college dormitory housed both the parents and the children, although they were located in different wings of the building. Each child had a separate room as free of distracting objects as it was possible to make it The general routine suitable for children of preschool age was carried out and the number of children was small enough to permit variations in the routine to meet individual differences The children were with their parents about an hour each day and the rest of the twenty-four hours, night as well as day, they were in the care of the nursery-school teachers. Two psychologists, a psychiatrist and a pediatrician, followed the daily progress of the children, and everything was done to provide optimum conditions for each child's growth.

Subjects of the Study

The number of children taking part in the study was small, 18 in 1931 and 16 in 1932, and the age range was from 17 to 62 months. It is probable that certain factors entered to make the group a selected one. The mean IQ of the group (as determined by the Kuhlmann test) was 128, which is considerably above average, and the health and general physical development of the children was at least average, as indicated by the physical examinations.

The question may rightly be raised whether the children brought to the Institute nursery school presented more so-called problems of adjustment than are found among the general population There is no way of answering this directly, but the opinion of the authors, based on several years' experience in the education of small children, is that problems were not found in greater numbers or greater intensity among the nursery-school children than among children in general.

The above are among the most obvious factors of selection and

it should be borne in mind that the results of a study of the sleep of these children may not be generally applicable to the sleep of other children of this age.

Comparison of the Results for Children of Different Chronological Ages

The results of this study then can be expected to throw some light on the amount of sleep taken by children of different ages. Table 1 shows that the mean total sleep[1] for the two-year-old children was 12½ hours, for the three-year-old children, 11 hours and 23 minutes; and for the four-year-olds, 10 hours and 57 minutes

TABLE 1
Comparison of the Amount of Total Sleep Taken by Children of Different Ages

Age groups→	(2-0) through (2-11)	(3-0) through (3-11)	(4-0) through (4-11)
Number of cases→	8	12	10
Range	11 12—12 59*	10 38—12 20	10 27—11.37
Mean	12 30	11 23	10 57
S D	36	32	17

*These figures are given in hours and minutes The computations were made to two decimal places but in making the tables the numbers were rounded out.

This means that the four-year-olds slept 1 hour and 33 minutes less than the two-year-olds and that the difference between the sleep of the three-year-olds and the two-year-olds was 1 hour and 7 minutes. In other words the younger children slept more than the older children, a fact which has long been recognized by parents as well as by authorities

Figure 1 shows the mean total sleep for each six-month age group It should be noted that there is a downward trend from the youngest to the oldest group in the amount of sleep taken but that this is not accomplished in regular steps.

The high negative correlation (—.83± .04) between chronological age and sleep given in Table 2 is also indicative of the tendency for the amount of sleep to decrease as the child grows older The

[1]Total sleep includes day and night sleep in 24 hours

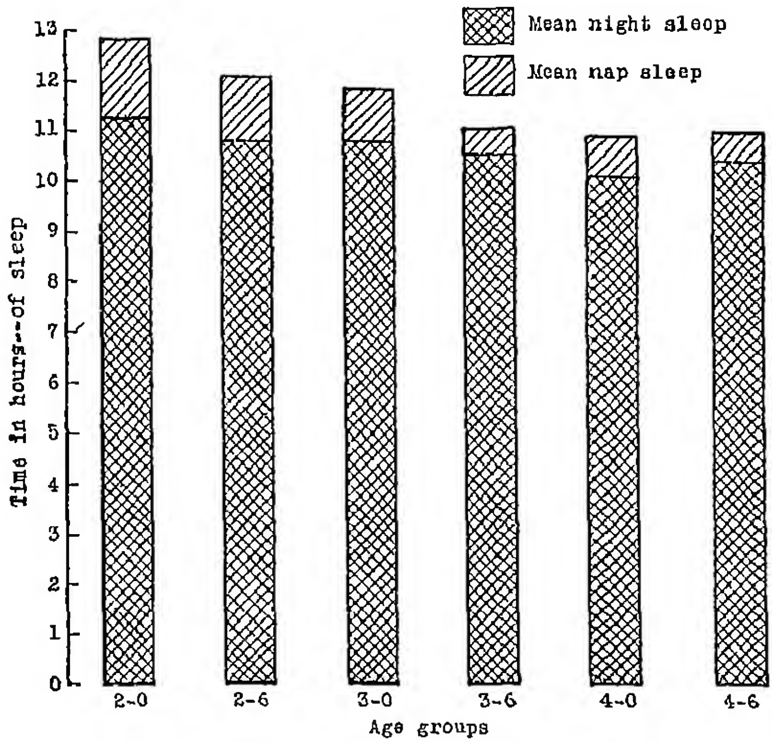

FIGURE 1
DAILY MEAN SLEEP

similar correlation (—.82±.04) between mental age and sleep also shows the expected relation, that, as mental age increases, the amount of sleep tends to decrease. Whether chronological or mental age is the more important factor in this connection can be studied by means of the partial correlations. It will be noted that when mental age is held constant, the correlation between chronological age and sleep drops to —.44±.10, while when chronological age is held constant, the correlation between mental age and sleep drops to —.37±.11. Although the difference is small, it may be interpreted as a slight indication that chronological age has a closer relationship with sleep than has mental age.

TABLE 2
CORRELATIONS BETWEEN TOTAL SLEEP AND CHRONOLOGICAL AGE, MENTAL AGE, AND HEIGHT-WEIGHT INDEX
Number of cases = 29

Correlation between	r	P.E.
CA and total sleep	—.83	±.04
MA and total sleep	—.82	±.04
CA and total sleep with MA held constant	—.44	±.10
MA and total sleep with CA held constant	—.37	±.11
Height-weight index and total sleep	+.055	±.13

An attempt was made to find whether the child's stage of physical development had a connection with the amount of sleep he took. No good objective measure of physical development is available and the height-weight index used in this study shows a correlation of only +.055±.13 with total sleep, which does not give evidence of any relationship. It may be, however, that another measure of physical development would show a significant relationship. All that can be concluded from the +.055 correlation of the Vassar study is that, for a small group of so-called well, normal children, no relationship was apparent between their height-weight indices and the amount of their sleep.

It will be interesting at this point to compare the results of the present study with those reported by other authorities. Table 3 gives the figures necessary for comparison with the Vassar Euthenics Institute results. Wagner's study, it should be remembered, was conducted on a group of orphanage children, and the recording was done by a trained worker. The studies by Foster, Goodenough, and Anderson and by Chant and Blatz were based on the results of parents' records over a shorter period of time but with a large number of cases. It is interesting to note that in all three age groups the range of means given by Wagner is lower than that of any of the other authorities.

For the two-year-old children, the amount of sleep taken in the Vassar nursery school agrees closely with that found by Foster, Goodenough, and Anderson and by Chant and Blatz, but for the three- and four-year-olds there is a noticeable discrepancy. The Euthenics Institute three-year-old children slept an hour and a half

TABLE 3

COMPARISON OF THE RESULTS OF SEVERAL STUDIES OF THE TOTAL SLEEP OF TWO-, THREE-, AND FOUR-YEAR-OLD CHILDREN

Age groups* →	(2-0)	(2-6)	(3-0)	(3-6)	(4-0)	(4-6)
Expert opinion (4)	12-15	12-15	10-14	10-14	10-14	10-14
Experimental evidence						
Vassar Institute of Euthenics	12 53	12 06	11 51	11 04	10 55	11 00
Foster Goodenough, and Anderson (2)	12 48	12 29 5	12 16.8	11 57 5	11 48 8	11 36 7
Chant and Blatz (1)	12 45		12 52		12 07	
Wagner (3)						
Range of means	11 00 to 11 54		10 25 to 11.56		10 09 to 11 55	

*The group is designated by the lower limit of the interval. For example, (2-0) includes all cases between two years, no months and two years, six months

less than the children of the same age reported by Chant and Blatz. The means for the sleep of the children in the Vassar study are from 46 to 70 minutes lower than the means for the corresponding ages reported by Foster, Goodenough, and Anderson and by Chant and Blatz

The full explanation of these discrepancies is not clear, but any one of several factors might account in part for them. Among the possible explanations, the method used in recording data for the study, the season of year at which the study was made, the economic class from which the children were selected, and the general physical condition of the children, undoubtedly all played their part in influencing the amount of sleep taken Chronological age, the one known common factor of these studies is only one of the many factors which have a bearing on the amount of sleep It is, therefore, not to be wondered at that there are wide differences in the amount of sleep taken by the different groups of children who were the subjects of these studies

Individual differences, too, play their part in explaining why the figures for the two-year-olds of the Vassar Euthenics Institute study agreed and those for the three- and four-year-olds did not with the amount slept by other children of their own age. If factors of selection of the Vassar children were responsible for the discrepancy, it would be expected that all three age groups would be similarly affected, which was not the case In the absence of any more plausible explanation, it may be permissible to assume that the individuality of the children was responsible for the lack of consistent disagreement.

Caution in applying the results of the Vassar College study generally should be exercised The number of children was small and the group undoubtedly selected. But perhaps the most important reason is that there is no way of telling whether the actual amount of sleep which the children took was the amount which they needed. Until the results of many studies have been accumulated, it will be wiser to use a child's own growth as the standard for the amount of sleep which he needs than to expect him to attain a hypothetical standard based on the amount of sleep taken by different children under different conditions.

Comparison of the Amount of Sleep and Presleep Time of Children of Different Ages. Up to this time, the discussion has been concerned with total sleep, i.e., the amount of combined day and night sleep in twenty-four hours. It will be profitable, now, to consider the length of the nap and the night sleep as well as the presleep time for both cases. The figures for these comparisons are given in Table 4.

An interesting point to notice is that the night sleep of the children in the four-year-old group showed a decrease over the amount taken by the children in the two-year-old group of 43 minutes while the total sleep decreased by 1 hour and 33 minutes This is not in agreement with the statements made by other authorities (2, p. 210) that night sleep remains practically the same for the two-, three-, and four-year-olds and that the decrease in amount of total sleep is accounted for by the decrease in amount of nap sleep with increasing age The results of this study indicate that *both* the night sleep and the nap were responsible for the decrease in total sleep, not merely the nap alone

One rather surprising fact shown in Table 4 is that it took the

TABLE 4
COMPARISON OF THE AMOUNT OF SLEEP AND PRESLEEP TIME OF CHILDREN OF DIFFERENT AGES

Age groups→	(2-0) through (2-11)	(3-0) through (3-11)	(4-0) through (4-11)
Number of cases→	8	12	10
Total sleep*			
Range	11 12—12.59	10 38—12 20	10 27—11.37
Mean	12·30	11·23	10 57
S D	36	32	17
Night sleep			
Range	10 23—11 20	10 00—11 17	9.50—10.43
Mean	11 01	10 41	10 18
S.D	21	26	18
Nap†			
Range	1·09—1 56	53—1·37	15—1.23
Mean‡	1·31	1.16	1 04
S D.	16	13	18
Presleep time at night			
Range	50—1 25	·33—1 27	32—1 33
Mean	1 00	1 00	1 04
S D.	11	15	20
Presleep time at nap§			
Range	:15— 47	23— 52	28—1 15
Mean	27	·38	40
S D.	12	9	13

*Total sleep includes day and night sleep in twenty-four hours The figures are repeated from Table 1.

†Nap means actual sleep Each child had a rest period of at least an hour every day but only on the days when the child went to sleep was it considered that he had had a nap These figures, therefore, apply only to the days on which naps were taken

‡Since many children did not actually sleep every day, this is the mean length of the nap on the days when a nap was taken It is not the mean of daily nap sleep comparable to the mean of night sleep It merely indicates how long the children slept when they did take naps

§This is the length of time taken to go to sleep on the days when a nap was actually taken

children in all three age groups approximately an hour to go to sleep at night and about a half an hour at nap time. This is longer than is usually expected by parents, nursery-school teachers, or specialists in child development, and, if further studies reveal the same tendency for young children to stay awake for long periods of time

after being put in bed, it may be necessary to revise some of the teachings concerning the formation of proper habits of sleep Is it not possible that a certain amount of presleep activity is inevitable and that the immediate sleep on being put to bed which is the aim of most child-training experts may be a false standard for young children under the present-day conditions? This study at least raises the question, and the authors hope that the results of experimental studies which will throw light on the matter will be forthcoming within a few years

The subject of naps merits more detailed consideration because of the amount of study which has already been given to it by nursery schools. The practical questions raised in all-day nursery schools which center around the nap period are legion and, although a great deal of experimental evidence is available, the results are in confusing disagreement. For this reason anything which throws light on the frequency, duration, and importance of the naps of preschool children is received with special interest

Turning again to Table 4, it can be seen that the average length of the nap when a nap was taken was something over an hour for the children in all three age groups. The two-year-olds slept only a third longer (1 hour and 34 minutes) than the four-year-olds (1 hour and 4 minutes) This evidence is in line with the belief that the nap is dropped out on the all-or-none principle Although the four-year-old children slept less frequently than the younger children, when they did take naps the length of the nap was only a third shorter than that of the two-year-olds who slept nearly every day.

It is not possible to compare the results of the Vassar Euthenics Institute study with many other studies because of the various ways in which the data about naps are treated However, the figures reported by Foster, Goodenough, and Anderson (2, p. 211) which are comparable are approximately a half hour higher than those of this study.

Table 5 gives the percentage of children taking naps different numbers of days a week In nearly every respect the figures for the children in the three- and four-year groups are closer together than those for the children in the three- and two-year groups The three-year-old children took naps 55% of the time, the four-year-olds 50%, and the two-year-olds 98% of the time Similarly, 25%

TABLE 5
COMPARISON OF THE PERCENTAGE OF NAPS FOR CHILDREN OF DIFFERENT AGES

Age groups→	(2-0) through (2-11)	(3-0) through (3-11)	(4-0) through (4-11)
	Percentage	Percentage	Percentage
Children taking naps			
Every day	50	8	0
At the rate of 5 days a week	37	25	40
At the rate of 3 or 4 days a week	13	25	10
At the rate of twice a week	0	17	30
At the rate of once a week	0	25	20
Percentage of days on which nap is taken			
Range	68-100	5-100	2-98
Median	98	55	50
Percentage of total sleep due to nap			
Range	7-14	.6-12	1-11
Median	12	6	6

of the three-year-olds and 20% of the four-year-olds took naps at the rate of once a week while none of the two-year-olds fell in this class. It should be noted that the range of nap sleep was wide for both the three- and the four-year-old children, so that there was a great deal of overlapping One three-year-old child took naps only 5% of the time while another child in the same age group took a nap every day Correspondingly, the range for children in the four-year group was from 2 to 98% Obviously there were significant individual differences within a chronological age group with respect to nap behavior Whether the agreement between the behavior of the three-year-old and four-year-old children is peculiar to the children in the Vassar group, or whether it is characteristic of children of this age in general, cannot be told. The mean chronological ages for each of the three groups were about at the midpoint of the interval and a year apart, but it is possible that in other respects affecting sleep the three- and four-year groups were closer together. Here, too, further study is needed before the question can be definitely answered

Summary of the Results of the Sleep of Children of Different Chronological Ages

The following main points stand out from a detailed consideration of the sleep of children of different chronological ages:

1. The mean total sleep for the two-year-olds was 12:30; for the three-year-olds, 11:23, and for the four-year-olds, 10:57. All three are below the amounts which it has previously been assumed that children of these ages should sleep

2. One hour was the mean length of time taken to go to sleep, regardless of chronological age. This is considerably longer than the usually accepted figure of 20 minutes

3. The mean length of the nap on the days when naps were taken ranged from an hour and a half for the two-year-olds to an hour for the four-year-old children. The percentage of the three- and four-year-olds' total sleep due to nap was just half that of the two-year-olds. The evidence supports the theory that naps are dropped out on the all-or-none principle

Comparison of the Sleep of Children in the 1931 and 1932 Schools

The second part of this study was concerned with evaluating the procedures used in the two summer schools In 1931, there was a great deal of confusion in the general administration of the school, which was particularly noticeable at bedtime Baths came after a very early supper, and it was thought that the children were overstimulated before going to bed. Furthermore, because of the fact that all meals were served in the nursery-school building and not in the dormitory in which the children slept, there was little opportunity for relief from the strain of group living throughout the day. Naps were taken in a room with as many as ten other children, according to the procedure of nearly all nursery schools.

The general dissatisfaction with the conditions of the 1931 summer school led to changes which made the 1932 school seem a decided improvement. Meals were served in the dormitory, baths came before instead of after supper which made the children seem more relaxed at the bedtime hour; and each child had his afternoon rest period alone in his own room A further change in procedure which it was thought might influence the children's sleep was instituted. The parents put the children to bed the first few nights and

everything was done to give the child that feeling of security in a strange place which is so essential to sleeping. Judged subjectively, the conditions of the 1932 summer school were superior to those of 1931. What effect did these conditions have on the children's sleep?

Chronological Age of Children in the 1931 and 1932 Schools. Before the data from these two years could be compared in an effort to find the answer to the above question, it was necessary to compare the children in the 1931 school with the children in the 1932 school with respect to chronological age. Only one child was a member of the school both years. Table 6 shows that .86 of a month was the actual difference between the mean ages, although the ranges showed considerable difference. The ratio of the real difference to the standard deviation of the difference[2] is .19, which is interpreted to mean that there are only 58 chances in 100 that the difference between the two means is greater than zero. In other words, the two groups can be assumed to be very closely comparable with respect to average chronological age.

Comparing the standard deviations for the two years, it was interesting to see (Table 6) that the figure for 1932 was almost twice as large as that for 1931. The ratio of the real difference to the standard deviation of the difference between these two measures[3] was 2.45, which, while high, was not indicative of a completely reliable difference. It is usually customary to regard anything under 3 as failing to indicate complete reliability, and in this study the 2.45 was interpreted to mean that there was a small chance that the difference between the standard deviations of the chronological ages of the children in the 1931 and 1932 schools was not a true difference. Therefore, with respect to means and standard deviations of chronological ages, the children in the 1931 school were considered fairly comparable with the children in the 1932 school.

The differences between the means of the two groups of children for mental age and IQ were larger, but, since the connection between chronological age and sleep is somewhat greater than that between

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[2] $\frac{D}{S.D_{diff}}$ when $S\,D_{diff} = \sqrt{S\,D_{(av\,1)} + S\,D_{(av\,2)}}$ and $S\,D_{(av)} = \frac{S\,D}{\sqrt{N-1}}$

[3] $\sigma_{\sigma_1 - \sigma_2} = \sqrt{\frac{\sigma^2_{dist\,1}}{2\,N_1} + \frac{\sigma^2_{dist\,2}}{2\,N_2}}$

TABLE 6
COMPARISON OF THE CHILDREN IN THE 1931 AND 1932 SUMMER SCHOOLS WITH RESPECT TO CHRONOLOGICAL AND MENTAL AGE AND IQ

	1931	1932
Chronological age		
Number of cases	18	16
Range	(2-1) through (4-5)	(1-5) through (5-2)
Mean	41 39 months	42 25 months
S D	7 98 months	15 58 months
Mental age*		
Number of cases	14	15
Range	(3-7) through (6-6)	(2-4) through (7-0)
Mean	57 5 months	55 0 months
S D	8 97 months	17.62 months
Intelligence quotient		
Number of cases	14	15
Range	104—151	91—159
Mean	129 43	126 53
S D.	12 94	17 03

*The Kuhlmann and Stanford revisions of the Binet tests were used to obtain mental age.

mental age and sleep, it was not considered profitable to prove the equivalence of the two groups statistically in this respect as it was for chronological age.

Total Sleep and Night Sleep of Children in 1931 and 1932 Schools. Assuming, then, that the children in the 1931 summer school were the same chronological age as the children in the 1932 summer school, it was interesting to learn that the mean of the children's total sleep was 31 minutes higher in 1932 than it was in 1931. The ratio of the actual difference to the standard deviation of the difference is over 6 so that it is practically certain that this difference between the means is a true difference

Table 7 gives the figures for a comparison of the children in the two schools with respect to night sleep, nap, and presleep time. The mean presleep time at night for the children in the 1932 school was 9 minutes less and the mean length of night sleep 26 minutes more than the corresponding figures for children in the 1931 school. With respect to conditions for night sleep, the figures seemed to give added weight to the subjective conclusions of the staff that the 1932 school was an improvement over the 1931 school.

Afternoon Naps in 1931 and 1932 Schools. The figures for the

TABLE 7
COMPARISON OF THE AMOUNT OF SLEEP AND PRESLEEP TIME OF THE CHILDREN IN THE 1931 AND 1932 SUMMER SCHOOLS

	1931	1932
No of cases	18	16
Total sleep		
Range	10 38—12 54	10.27—13 51
Mean	11 21	11 52
S D	38	60
Night sleep		
Range	9 50—11 20	10 09—11 46
Mean	10.29	10 55
S D	26	27
Nap*		
Range	53—1 39	15—2 06
Mean	1 14	1 22
S D	12	26
Night presleep time		
Range	32—1 33	38—1 28
Mean	1 05	56
S D	17	14
Afternoon presleep time		
Range	16—51	14—75
Mean	35	36
S.D	11	14
Percentage of total sleep due to nap	Percentage	Percentage
Range	1—12	1—14
Mean	7.5	7
Percentage of number of naps taken during the six weeks		
Range	15—100	2—100
Mean	86	58 5

*See explanatory footnote † to Table 4

comparison of the procedures used at the afternoon rest period showed disappointingly small differences The mean length of the nap on days when a child actually slept was only 8 minutes longer in 1932 than in 1931, and the mean presleep time at nap differed by only 1 minute. It should be recalled that in 1931 the children had their afternoon rest periods in a room with several other children, while in 1932 each child was in an individual room for the

corresponding time It was, therefore, surprising to find that the children took about as long to go to sleep whether they were in a room alone or with several other children, and that, when they did go to sleep, they slept only 8 minutes longer in individual rooms.

Studying the problem of the nap from other angles, it can be noted from Table 6 that, when the percentage of total sleep due to nap was figured for each child in both schools, the medians were only a half of one per cent apart. Furthermore, when the number of times the child actually slept at the rest period was figured as a percentage of days he was present at school, it was found that the median for 1931 was considerably higher than that for 1932

These differences, for the most part small and in some cases contradictory, do not support the assumption that the conditions for the afternoon nap in the 1932 school were superior to those in the 1931 school. The conclusion reached by other investigators that resting in a room with other children is prejudicial to an afternoon nap is not borne out by the results of this study

Although it has been shown that the difference of 31 minutes between the mean total sleep of the children in the 1932 school over that of the children in the 1931 school is a true difference statistically, an analysis of the evidence from another point of view will be profitable.

If the 1932 summer school was more successful than the 1931 one with respect to the children's sleep, when was the improvement noticed? The expected picture for each year, according to expert opinion, would be something like this a low average sleep the first few days while the children were getting adjusted to the unfamiliar surroundings of the Institute, to sleeping away from their mothers at night, and to the presence of so many strange people; then a gradual rise in amount of daily sleep taken so that the last week of the summer school would show considerable improvement over the first week in this respect It was anticipated that both years would show the same general trends but that the 1932 figures would be somewhat higher than those for 1931.

Weekly Averages for 1931 and 1932 Schools Table 8 gives the weekly averages of the total sleep of children in the two schools. It can be seen that in 1931 the average for the sixth week was 24 minutes higher than the average for the first week In the four middle weeks, there was remarkable agreement in the averages, since

TABLE 8
WEEKLY AVERAGES* OF TOTAL SLEEP OF CHILDREN IN 1931 AND 1932 SUMMER SCHOOLS

	1931	1932
Number of cases	18	16
First week of school	11 09	11 55
Second week	11 23	11.55
Third week	11 20	11 44
Fourth week	11 25	11 52
Fifth week	11.20	11 53
Sixth week	11 33	11 47

*Weekly averages were obtained from the daily averages for each seven days of the school

there was only 5 minutes difference between the lowest and the highest of them. The improvement which was shown was not, then, a gradual increase in amount of sleep taken through the six weeks of the Institute but an increase of 14 minutes between the first and the second week and another 13-minute increase from the fifth to the sixth week But improvement there undoubtedly was, and it may be that this was directly traceable to the formation of better habits of sleep on the part of the children In this respect, the actual figures for the 1931 school show some resemblance to the expected picture.

Looking at the weekly averages for 1932, it is noted that the children slept 8 minutes less the sixth week than they did the first week of the Institute. The averages for the first two weeks were the same and that for the third week was the lowest, being only 11 minutes lower than the week with the highest average There is, therefore, no evidence of the period of initial adjustment for the children, nor is there any proof of improvement in the amount of sleep taken by the children during the six weeks of the 1932 Institute. Obviously, the children were sleeping as much when they came to the school as they were after six weeks under its supposedly optimum conditions for sleep The figures for 1932 certainly do not fit the expected picture in any respect

A comparison of the weekly averages for the two years shows that consistently for every week the 1932 averages were higher than those for 1931. In fact, the lowest 1932 average is 11 minutes above the highest 1931 average The difference is unmistakably in favor of 1932, as it was in a comparison of mean total sleep of the

TABLE 9
DAILY AVERAGES* OF TOTAL SLEEP OF CHILDREN IN 1931 AND 1932 SUMMER SCHOOLS

	Number of cases	1931	Number of cases	1932
First day of school	13	11 11	7	11.45
Second day of school	15	11 00	10	11.21
Third day	17	11 15	12	11:53
Fourth day	18	11 00	13	12·09
Fifth day	17	11 09	13	12·13
Sixth day	18	11 09	13	12 10
Seventh day	16	11 15	15	11.40
Eighth day	18	11 30	15	11.52
Ninth day†	18	11·32	16	12·23

*Daily averages are the averages of the sleep of all children in the group for the same 24-hour period

†Since the 1932 group of children was not complete until the ninth day, it seems advisable to include the eighth and ninth days

children for the whole six weeks of the Institute.

Table 9 gives the daily averages for the first few days of the school in 1931 and 1932 and it is evident that on the opening day of school the children slept more in 1932 than they had on the corresponding day in 1931. Figure 2 shows that this superiority in amount of sleep taken was maintained throughout the six weeks of the Institute

Tentative Explanations of the Results of the 1931 and 1932

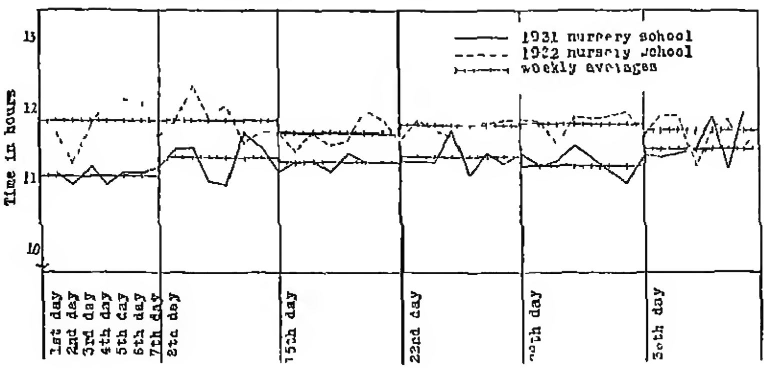

FIGURE 2
DAILY MEAN TOTAL SLEEP FOR CHILDREN IN THE 1931 AND 1932 NURSERY SCHOOLS

Schools. In trying to find the explanation for the greater amount slept by the children in 1932, it should be remembered that chronological age was the only factor for which the children in the two schools were equated Other less easily measured factors, such as emotional stability or general health, may have influenced the sleep of the children to a greater extent than chronological age In the second place, amount of sleep, with which this study was concerned, was only one measure of a child's habits of sleeping Independence in going to sleep, depth of sleep, and reduction of restlessness are all important phases of sleep, and there is always the possibility that improvement in these took place, although no way of measuring them was used for this study

It will be desirable at this point to summarize the significant facts brought out by a comparison of the weekly and daily averages for the two years (1) The sleep of the children in the 1931 school showed the effects of the expected period of adjustment at the beginning and improvement during the six weeks (2) The sleep of the children in the 1932 school showed neither the effects of a period of adjustment nor any improvement for the duration of the time. (3) From the opening day of school, the amount of sleep taken by the children in 1932 was greater than that taken by the children in 1931

Several explanations for these facts suggest themselves, but no one outstanding cause is obvious It is, therefore, to be expected that there will be differences of opinion in attributing the phenomena to their various causes. The following tentative explanation is offered in the hope that further studies will soon test its acceptability The better conditions of the 1932 school were responsible for the greater amount of sleep of the children that year The effect of the better environment was noticeable on the opening day of school But these same conditions succeeded in enabling the children to take, from the very beginning, only the amount of sleep they needed They did not teach the children to sleep beyond a certain point Conversely, the unfavorable conditions of the 1931 school were responsible for the fact that the children slept less that year. The influence of these conditions was greatest at the opening of school and decreased during the six weeks There is no way of telling whether the children were sleeping the maximum amount at the close of school or whether there would have been a further decrease if the school had been in session longer

The effect of environmental conditions on children's sleep has long been recognized Unfavorable conditions undoubtedly do keep children from sleeping as much as they should. But favorable conditions do not make children increase the amount of their sleep to an unlimited degree.

The above conclusions appear to limit the possibilities of the control of the amount of the sleep of preschool children by environmental conditions They suggest that external conditions must at least share the honors of getting children to sleep the required amount with some cause more fundamental to the organism It is not only possible but probable that children sleep as much because their bodies need it as because the room is quiet, restful, and without distractions And, conversely, it is probable that one reason why children do *not* sleep when anxious adults would have them do so is because they do not need sleep at that moment rather than because the external conditions are not quite right

Summary. A comparison of the sleep of the children in the 1931 school with that of the children in the 1932 school yielded the following results:

For the afternoon rest period:

1 It took the children as long to go to sleep whether they were in individual rooms or in a room with several other children

2. The mean length of the nap was 8 minutes longer when the rest period was taken in individual rooms

3. More naps were taken by the children in the 1931 school when they had their rest periods in a room with several other children than by the 1932 children who rested in individual rooms.

4. There was no evidence that resting in a room with other children was prejudicial to the afternoon nap.

For the amount of total sleep:

5 The mean for the 1932 children was 31 minutes higher than that for the children in the 1931 school.

6 In 1931, an improvement of 24 minutes from the mean of the first to the mean of the sixth week was found.

7. In 1932, no improvement was shown from the first to the sixth week

8 In explanation of these facts, the question was raised whether the influence of environmental conditions on the amount

that children sleep was not more limited than had previously been supposed. It was further suggested that some organic factor might determine to a considerable extent the amount that the children slept.

Constancy of Amount of Sleep Taken

The third part of this study concerned itself with an attempt to determine the reliability of the so-called organic factor as a motivating force for children's sleep The data did not permit of any exact measure of a child's need for sleep, but a study of the constancy of the amount of sleep taken by individual children throws some light on this problem

The literature on the subject of sleep both for adults and children mentions rhythm as a factor in sleep The rhythm which is referred to is the rhythm of daily sleeping and waking and the belief is that once the habit has been formed an individual will tend to fall asleep at approximately the same time each evening and to waken at the same hour each morning It is to be regretted that the Vassar Euthenics Institute study did not provide data concerning children's rhythms of sleeping and waking, but during the summer schools so much stress was laid on forming proper habits of sleep that there was no opportunity to study the effect of existing rhythms. If, for instance, a child wakened too early in the morning, conditions were altered on succeeding days in an attempt to have him sleep longer. Every effort was made to establish an optimum rhythm rather than to study the reactions of the child under the existing one

For these reasons, no contribution to our understanding of the sleeping and waking rhythm is forthcoming from the Vassar study, but some interesting conclusions concerning the constancy of the amount of sleep taken over a six-weeks period are available The data for this part of the study were obtained in the following way:

As the records were being charted daily it was noticeable that the fluctuations in amount of a child's total sleep from day to day were great One child varied as much as 3 hours and 45 minutes between the day with the most sleep and the day with the least sleep, and an hour and a half was the smallest variation found for any child This variation, it should be remarked, was sometimes up, sometimes down, but in only two cases was it distinguishable as consistent loss or gain over the amount of sleep the child took at the opening of

the school. All that was clear was that rises or drops of an hour and a half or two hours occurred with such frequency that they began to be regarded as the usual rather than the unusual thing.

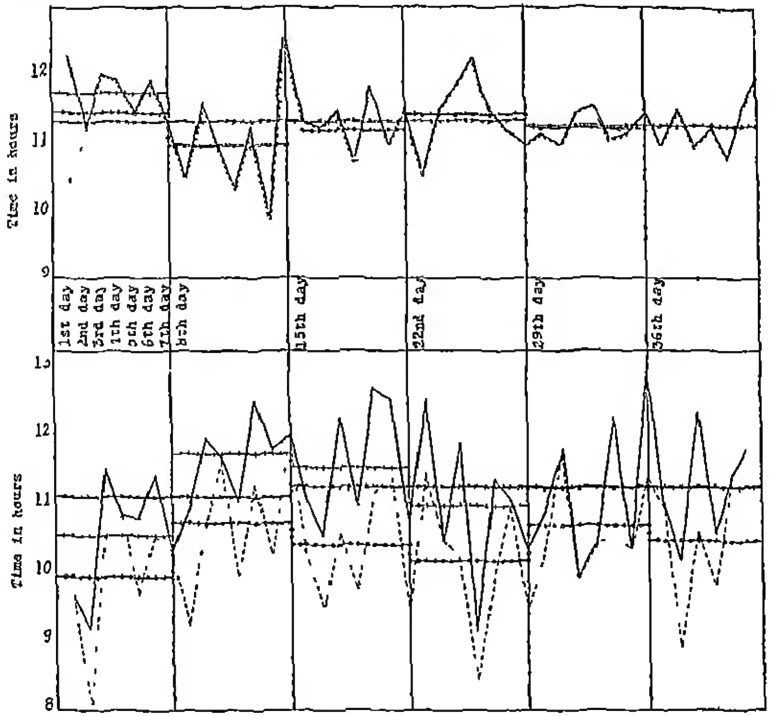

FIGURE 3

Charts of Daily Total Sleep Taken by Two Children in the Summer School

Upper—1932 CA, 3 yrs. 6 mos
MA, 4 yrs. 9 mos.
Lower—1931 CA, 2 yrs. 9 mos

———— Daily total sleep
– – – – Daily night sleep
+++++ Weekly average—total sleep
•–•–• Weekly average—night sleep
×××× Biweekly average—total sleep

The space between the dotted line and the solid line shows the amount of nap taken.

A further interesting point brought out by the study of the daily variations in the amount of sleep taken by the children under the conditions of the Vassar nursery school is that so-called "lost sleep" was not made up all at once. An inspection of the accompanying graphs, Figure 3, shows that it was unusual for a child's sleep to jump directly from the highest to the lowest point. Usually the increase or decrease was by steps, over several days. For example, for two or three days a child took more than his average amount of sleep or less as the case might be. If this is universally so, it is unsafe to assume that an unusually long sleep one night or long nap one afternoon will mean a shorter period of sleep the following night. It may mean quite the opposite.

Theories of a daily rhythm of sleeping and waking were certainly not borne out by the data. Administrative changes could not be counted on to explain both the losses and the gains in any understandable way. Two questions then became pertinent: (1) how serious were the fluctuations? and (2) what caused them? An attempt was made to discover whether there was a tendency for the organism to maintain its sleep balance in spite of interruptions in the daily rhythm.

The search for other patterns than the 24-hour one was made by computing the averages for all the possible combinations of days from 2 to 21. For example, for a child, the average of every 2 days' sleep, 3 days' sleep, 4 days' sleep, etc., for the six weeks of the Institute was studied and the weekly average selected for further study. This choice was made because the variations between the averages for 2 days either side of the 7-, 14-, and 21-day periods were so close to the weekly, biweekly, and triweekly averages as to be unimportant. No claim is made that these 7-day units are the ideal ones. They were simply the most convenient for this study. Future studies will undoubtedly reveal more definite patterns of varying length which the present data failed to disclose.

For this comparison of the weekly, biweekly, and triweekly averages, the data for one child whose records were incomplete were excluded. Table 10 gives the range between the highest and lowest average for each child.

Assuming that 15 minutes or less was a negligible difference, it can be seen that 12 of the 32 children took the same amount of sleep every two weeks. Nine more children took the same amount

of sleep every three weeks. Twenty-one children, therefore, or 66%, remained constant with respect to sleep taken over a two- or three-week period.

TABLE 10

DIFFERENCES BETWEEN THE WEEKLY, BIWEEKLY, AND TRIWEEKLY AVERAGES

CA	Weekly	Bi-weekly	Tri-weekly	CA	Weekly	Bi-weekly	Tri-weekly
1-5	38*	8†	13‡	3-8	29	17	3
1-8	64	31	18	3-10	57	35	32
2-0	55	12	14	3-10	27	24	5
2-2	30	13	6	3-11	31	9	13
2-2	19	17		4-0	40	8	12
2-5	53	22	20	4-1	30	12	11
2-6	54	23	18	4-2	35	15	8
2-6	65	36	27	4-3	75	39	24
2-6	42	20	22	4-3	30	11	16
2-9	69	9	7	4-6	40	30	22
3-0	67	51	32	4-8	51	12	17
3-0	39	23	11	4-8	45	20	15
3-1	33	14	8	4-11	17	12	11
3-3	48	36	15	4-11	65	28	6
3-5	27	19	12	5-2	78	66	52
3-6	45	5	2	5-2	27	13	
3-6	83	46					
3-6	47	28	25				

*This means that the difference between the week in which the child slept the least and the week in which he slept the most was 38 minutes

†This means that the difference between the child's sleep the first two-week period, the second two-week period, and the third two-week period was 8 minutes

‡This means that the difference between the child's sleep for the first three weeks and his sleep for the last three weeks was 13 minutes.

If 30 minutes is assumed to be a negligible difference, a further inspection of Table 10 reveals the fact that 9 children took within 30 minutes of the same amount of sleep every week, 17 every two weeks, and 4 every three weeks Only 3 cases varied more than 30 minutes in their triweekly averages and 2 of these exceeded the limit by only 2 minutes, which is so small as to be unimportant In other words, all but one child remained acceptably constant in the amount of sleep taken in either one-, two-, or three-week periods.

It should be further noted that only one child showed a consistent improvement in amount of sleep taken weekly during the Institute, so that his average the sixth week was 1 hour and 5 minutes more than his first week's average Conversely, another child showed a

decrease of 1 hour and 16 minutes in amount of sleep taken weekly from the first to the sixth week. But these children were exceptions, the majority fluctuated from week to week, sometimes showing a gain sometimes a loss but without a consistent tendency in either direction.

This fact becomes increasingly significant when it is recalled that the situations in which these figures were gathered were in no sense experimentally controlled As has been previously pointed out, the only control was that of attempting to secure optimum sleep conditions for each child in so far as it was compatible with the interests of the group Procedures were varied to suit the needs of the moment, some days a child was awakened from his nap, other days he was allowed to sleep as long as he wished, occasionally a child was given his supper in bed or an extra period of rest The interesting thing is that, with all these changes, the individual children took practically the same amount of sleep over a certain period of time

What explanations are suggested by these facts? The usual ones of administrative or teaching procedures cannot satisfactorily account for both the losses and the gains The importance of temporary conditions such as the child's physical condition or his immediate environment fades into insignificance when it is noted that the effects were evidently temporary, too A child may have slept less one day because of illness or outside disturbances, but, since he came out even in the satisfaction of his sleep needs at the end of two or three weeks, the effects of the temporary condition were evidently temporary, too, with respect to his sleep Something more fundamental than the influence of immediate conditions seems to be at work and the explanation may lie in the physiological drive of the organism for rest and sleep

Children's sleep may be not the "volatile habit" that it has been assumed to be but rather a dependable factor which remains constant in spite of changing conditions There is, of course, further need for more studies of children's sleep over longer periods of time, but the evidence from the Vassar study at least raises the question whether the organic drive for sleep is not one of the most important factors in children's sleep.

This idea is by no means new Psychologists have long recognized not only the existence but the insistence of the physiological drives,

but specialists in the field of child training have been slow to rely on them for motivation of children's behavior.

There is ample evidence in the literature that many authorities are in agreement with the underlying theory. For example, Dr. Woolley has stated that "children living under healthful, wholesome conditions will take as much sleep as they need and waken when they have had enough" (4, p 47). But there is little evidence that the authorities expect this theory to work out in practice The advice which they give for the formation of proper sleep habits does not conform to the theory which seems to be proven by this Vassar study.

The reasons for this discrepancy between theory and practice can only be touched on here For one thing, the recent emphasis on habit formation has tended to obscure the other causes of children's behavior and, since the physiological drives are seemingly less under the control of adults than the immediate environmental conditions, their significance has been minimized accordingly.

The *laissez faire* policy toward sleep advocated by this theory should be adopted cautiously and with due regard for its limitations A similar theory with respect to children's eating has been in practice in recent years with notable success. There is no reason for assuming that the application of this theory to children's sleep will be productive of more difficulties Is it not time, perhaps, that the same attitude was adopted toward sleep that is taken toward eating? "No forcing, no fussing" would be as good a motto to apply to the formation of proper habits of sleeping as of eating. Sleep meets a physiological need of the organism just as food does, missing a nap or taking a shorter night's sleep than usual is no more upsetting than missing a meal Adequate environmental conditions are essential, but once they have been provided the adults' responsibility and worry should end Just as adults provide for the children opportunities for adequate food intake each day, so they should provide optimum conditions for sleep. And, under these conditions, the physiological drive for sleep can be counted on to get the child to sleep when he needs it. Whether the child sleeps or not at any time is a matter of no concern for the moment, but if a careful study reveals a continuous refusal to satisfy his sleep needs adequately then the symptoms of maladjustment should be investigated, as in any other behavior difficulty.

There is then no justification in the results of this study for allow-

ing irregular bedtimes or for dropping the afternoon rest period for children of preschool age In fact, all the evidence tends to emphasize our inability to estimate in advance how much sleep a child needs at any particular time and to strengthen the belief that it is the adult's responsibility so to arrange the environment that at regular periods the child can sleep if and when he needs to.

Summary and Conclusions

The present study is concerned with sleep of 34 children between the ages of 17 and 62 months at the Vassar College Institute of Euthenics nursery school during the six-weeks summer sessions of 1931 and 1932 No experimental situation was set up, but every effort was made to provide optimum conditions for the sleep of each individual child. The records were kept by the teachers in the ordinary course of their duties and were carefully supervised by the directors of the school The data were limited to the quantitative aspects of sleep

It was found that the two-year-old children slept 12 hours and 30 minutes out of the 24; the three-year-olds, 11 hours and 23 minutes; and the four-year-olds, 10 hours and 57 minutes. One hour was the mean length of time to go to sleep at night, regardless of chronological age. With respect to naps, there was evidence that the nap tends to drop out on the all-or-none principle, as has been previously assumed

A comparison of the procedures used in 1931 with those used in 1932 appeared to indicate that, with the better environmental conditions of 1932, the children tended to take more sleep, but that the conditions were powerless to increase this beyond a certain amount. There was no evidence that taking an afternoon rest in a room with several other children was prejudicial to sleeping.

It was found from a study of the constancy of the amount of sleep taken by individual children that the daily fluctuations were great but that the weekly, biweekly, and triweekly averages were in closer agreement The data pointed to the conclusion that over a longer period of time than 24 hours most of the children maintained an acceptable constancy in the amount of sleep they took

The question was raised whether the physiological drive for sleep was not more important than had previously been supposed and, conversely, whether the influence of external conditions was not of

less importance than the literature on child training has indicated. The suggestion was made that it was perhaps time that the attitude of adults toward children's sleep was revised in much the same way that the attitude toward eating has recently been changed.

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Institute of Euthenics
Vassar College
Poughkeepsie, New York

LE SOMMEIL DES JEUNES ENFANTS
(Résumé)

On a étudié les aspects quantitatifs du sommeil à l'école pour petits enfants du Vassar College Institute of Euthenics en 1931 en 1932. Les sujets ont été trente-quatre enfants entre l'âge de dix-sept et de soixante-deux mois. Les données ont été obtenues par les maîtresses de l'école quand elles ont fait leurs devoirs ordinaires de soigner les enfants vingt-quatre heures par jour.

Le sommeil total moyen a été de 12 30 heures pour les enfants âgés de deux ans, de 11 23 pour ceux âgés de trois ans, et de 10 57 pour ceux âgés de quatre ans.

Le sommeil des enfants en 1932, quand les conditions ont été plus favorables, a été plus long qu'en 1931, mais l'influence des conditions du milieu a été plus limitée qu'on n'a supposé antérieurement.

Les fluctuations quotidiennes dans le sommeil des enfants individus ont été grandes, mais les moyennes pendant une semaine, deux semaines et trois semaines se sont étroitement accordées. Pendant une période durant plus de vingt-quatre heures, la plupart des enfants ont maintenu une constance acceptable dans la longueur de leur sommeil. On s'est demandé si l'impulsion physiologique vers le sommeil est plus importante que l'on n'a pensé antérieurement, et on a suggéré qu'il faut changer l'attitude des adultes à l'égard du sommeil des enfants de la même manière qu'on a changé l'attitude à l'égard de leur nourriture.

Reynolds et Mallay

DER SCHLAF JUNGER KINDER
(Referat)

In der Kleinkinderschule des Vassar College Institute of Euthenics wurden in Jahre 1931 und 1932 quantitative Erscheinungen des Schlafes studiert Die Versuchspersonen waren vierunddreissig Kinder im Alter von siebzehn bis zweiundsechzig Monaten Die Angaben wurden durch Lehrerinnen der Kleinkinderschule wahrend der ordentlichen Pflege der Kinder vierundzwanzig Stunden im Tag gesammelt

Die mittlere Gesamtdauer des Schlafes war 12 Stunden 30 Minuten fur zweijahrige, 11 Stunden 23 Minuten fur dreijahrige und 10 Stunden 57 Minuten fur vierjahrige Kinder

Im Jahre 1932 als die Bedingungen gunstiger waren, war der Schlaf der Kinder besser als im Jahre 1931, aber der Einfluss der Umgebung war beschrankter als fruher angenommen wurde

Tagliche Schwankungen im Schlaf einzelner Kinder waren gross, aber wochentliche, zweiwochentliche und dreiwochentliche Durchschnitte stimmten nahe uberein Bei einem langeren Zeitabschnitt als vierundzwanzig Stunden, wiesen die meisten Kinder eine erwunschte Konstanz bezuglich der Schlafdauer auf Die Autoren stiessen dadurch auf die Frage, ob der physiologische Drang nach Schlaf wichtiger sei als fruher angenommen wurde und schlagen vor, dass die Eltern ihre Haltung gegen Kinder bezuglich des Schlafs andern, so wie die Haltung dem Essen gegenuber kurzlich geandert wurde

Reynolds und Mallay

THE MUSICAL ABILITY OF MOUNTAIN CHILDREN AS MEASURED BY THE SEASHORE TEST OF MUSICAL TALENT*

From the Department of Psychology of the East Tennessee State Teachers College

LESTER R WHEELER AND VIOLA D WHEELER

During the past few years there has been a remarkable revival of interest in the music of the Southern Highlands Students of folk-lore are collecting and preserving the folk-songs and ballads of mountain people Talking machine and radio bring to the outside world the melancholy strains and weird tales of mountain songs, some of which were sung and lost in England centuries ago The ability of the Highland people, which has enabled them to preserve the antique ballads of their mother country as well as make many excellent new ones, seems rather remarkable. It is the purpose of this study to measure objectively the musical ability of the mountain people

Because the majority of studies of musical talent do not fall directly within our problem, we shall omit any detailed review of the literature. As far as we can learn no attempt has been made to study the musical ability of mountain people by objective methods. There are many problems to consider in selecting a scale for measuring this trait. For our purpose, wide standardization and use of a test is necessary for comparisons Also the lack of musical instruction among the mountain children makes it necessary to use a test involving the least amount of training We feel the Seashore Test of Musical Talent most nearly approaches these requirements, but in using it we read our results with reservations, mindful that the test has discrepancies and many adverse criticisms.[1] We make no attempt to defend

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[1]Seashore (43) gives the history of research on music at the University of Iowa for the past 25 years The technique and construction of the tests is clearly given, and one feels that valuable service has been rendered in an attempt to measure music ability of adults and children Farnsworth (8) gives a good discussion of the studies which have been made on the reliability and standardization of the Seashore-Kwalwasser battery He finds the tests should be of value in certain group studies and might be of

the validity of the test, but feel that for our purpose it is the best test available. Its results at least give us data for comparisons in the six elements—pitch, intensity, time, consonance, tonal memory, and rhythm. As to how great a part these factors play in musical talent we do not know.[2]

Several other difficulties arise when studying the results of the Seashore Test. While a review of a number of studies shows that the test is fairly well standardized, the norms seem high when compared with children in other investigations. In W. S. Larson's (27) study of four groups of children in public-school music, only the members of the senior-high-school advanced orchestra compare favorably with the norms. Ruth Larson (26) studied pupils beginning music in the eighth grade, and her results compare more favorably with those of W. S. Larson for the same selection of cases than with the norms. Groups measured by Gray and Bingham (15), Garth (11), Lenoir (28), and others show similar results. Larson's final norms are not materially different from the original, and the same criticism might be made of her work. Perhaps a wide enough range of public schools has not been included in the process of standardization.

Another difficulty in using the test is its cumbersome length. Lanier (22, 24) has pointed out that the test could be reduced in length and not materially affect its usefulness. This fact seems plausible in studying the results on the mountain children of this study. Another weakness of the test is the lack of standards in terms of scores and adequate reliability measures. This materially handicaps accurate comparisons of studies by different investigators. We

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value for general classification purposes. For other studies on the reliability, validity, prognostic, and predictive value of the Seashore test see Brown (4), Highsmith (17), Lanier (23), Mursell (33), Schoen (37), Brennan (2), Tilson (51), Wilson (52), Wright (54), Heinlein (16), Stout, (50), and Gaw (12). Space does not permit a discussion of these references.

[2]Lowry (29) has discussed some of the fundamentals underlying musical ability. *He points out two phases of music, technique and interpretation,* which must be the basis for a music test. He believes that an individual may interpret and appreciate music and not be skilled in its technique. Farnsworth (7) gives a comprehensive discussion of the effects of nature and nurture on musicality. He reviews studies on music in relation to genetics, intelligence, chronological age, and training. He concludes the problem is still a moot question that cannot be answered by mendelian counts on the human race, nor while social inheritance continues to undermine the values of a study of musical families.

TABLE 1

MUSICAL TALENT OF MOUNTAIN CHILDREN AS MEASURED BY SEASHORE TESTS

	Grade 5	6	7	8	Total
I. Sense of Pitch					
No. of cases	123	109	96	91	419
Median	51.41	52.72	53.85	57.95	53.22
P.E. (med.)	.869	1.052	1.037	1.611	.573
Q-3	60.56	61.71	61.25	69.07	62.63
Q-1	45.15	44.14	45.00	44.42	43.90
Q	7.71	8.79	8.13	12.28	9.37
II. Sense of Intensity					
No. of cases	123	109	96	91	419
Median	69.08	76.25	82.41	81.88	78.59
P.E. (med.)	1.039	1.421	.810	1.334	.589
Q-3	79.42	89.30	87.50	92.70	85.33
Q-1	60.98	65.57	75.00	72.34	66.04
Q	9.22	11.87	6.25	10.18	9.65
III. Sense of Time					
No. of cases	125	102	117	102	446
Median	60.28	63.33	69.22	66.78	64.18
P.E. (med.)	.834	.832	.919	1.129	.494
Q-3	67.62	70.63	74.95	77.75	73.21
Q-1	52.68	56.17	59.08	59.50	56.50
Q	7.47	6.73	7.94	9.13	8.36
IV. Sense of Rhythm					
No. of cases	121	102	94	90	407
Median	60.45	62.61	69.00	62.22	61.78
P.E. (med.)	.775	1.251	1.065	1.241	.491
Q-3	66.19	69.53	70.90	71.56	68.98
Q-1	52.55	49.32	54.38	52.75	53.34
Q	6.82	10.11	8.26	9.41	7.82
V. Sense of Consonance					
No. of cases	120	103	130	103	456
Median	60.61	63.44	62.66	63.08	62.34
P.E. (med.)	.794	1.032	.857	.688	.430
Q-3	65.39	72.23	69.61	68.75	69.05
Q-1	51.47	55.38	53.97	57.58	54.34
Q	6.96	8.38	7.82	5.59	7.36
VI. Tonal Memory					
No. of cases	123	103	93	91	410
Median	31.95	33.30	34.51	40.66	34.43
P.E. (med.)	.862	1.065	.791	.869	.593
Q-3	38.75	42.11	42.28	46.25	42.75
Q-1	23.44	24.75	30.05	32.68	26.43
Q	7.65	8.66	6.12	6.79	8.16
Average performance on all six tests	55.63	58.60	61.93	62.09	59.26

feel that Ruth Larson has not contributed a great deal in her revision of the norms because she has added little to the original data except number of cases. Reliable standards should be given for the sixth and seventh grades. Adequate reliability measures should accompany all norms. Further research needs to be done on a number of phases of the test.

During the spring of 1932 the writers gave the Seashore Test of Musical Talent to 456 mountain children of East Tennessee in Grades V, VI, VII, and VIII. All these children were found in the public schools of the mountains, and were tested under ordinary public-school conditions. They constitute an unselected group of mountain children. The scoring of all the tests was carefully checked. Table 1 shows the number of cases and the statistical measures used to interpret this investigation.

Table 2 shows a comparison of the mountain children with Seashore's norms for pitch, intensity, time, rhythm, consonance, and tonal memory. The data indicate that the mountain children are significantly below the norms in pitch and tonal memory, but with less differences in the other measures. Since Seashore's norms have

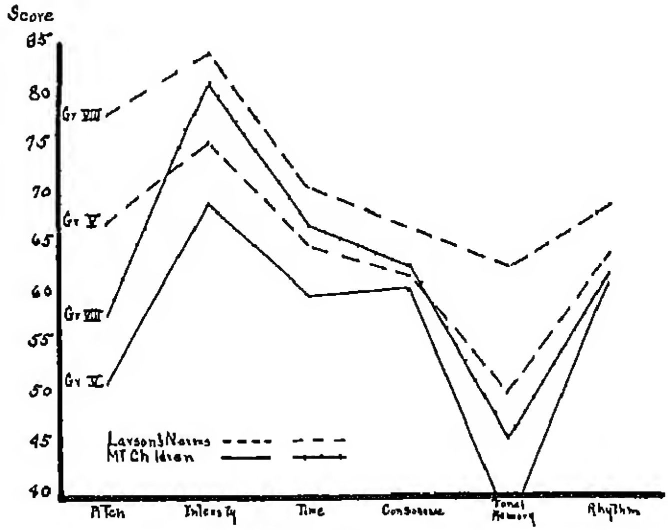

FIGURE 1

Musical Talent of Mountain Children Compared with Larson's Norms

TABLE 2
COMPARISON OF THE MEDIAN ACHIEVEMENT OF MOUNTAIN CHILDREN WITH SEASHORE'S NORMS

		Grade 5	6	7	8	Av.
I	Sense of Pitch					
	Seashore	66 50	70 00	74 00	76 75	71 82
	Mountain children	*51 41±0 869*	*52.72±1 052*	*53 85±1 037*	*57.95±1 611*	53 98±0 573
	Difference	15 09	17 28	20.15	18 80	17 83
II	Sense of Intensity					
	Seashore	73 66	77 00	80 40	82 80	78.47
	Mountain children	69 08±1 039	76.25±1 421	82 41±0 810	81 88±1 334	77 41±0 589
	Difference	4 58	.75	—2 01	92	1 06
III.	Sense of Time					
	Seashore	64 20	66 40	69 00	71 00	67 65
	Mountain children	60.28±0 834	63 33±0.832	69.22±0 919	66 78±1.129	64 90±0 494
	Difference	3 92	3.07	— 22	4 22	2 75
IV	Sense of Rhythm					
	Seashore	64.75	66 40	68 20	70 00	67.34
	Mountain children	60 45±0 775	62 61±1 251	69 00±1.065	62 22±1 241	63 57±0 491
	Difference	4 30	3 79	— 80	7 78	3 77
V	Sense of Consonance					
	Seashore	61 00	63 00	64 75	62 20	62 74
	Mountain children	60 61±0 794	63 44±1 032	62 66±0 857	63 08±0 688	62 45±0 430
	Difference	39	— 44	2 09	— 88	.29
VI	Tonal Memory					
	Seashore	50 00	56 00	61 00	64 67	57 92
	Mountain children	31 95±0 862	33 30±1 065	34.51±0.791	40 66±0 869	35 11±0 593
	Difference	18 05	22 70	26 49	24 01	22 81

TABLE 3

PERCENTAGE OF MOUNTAIN CHILDREN ABOVE AND BELOW THE MEDIAN OF SEASHORE'S NORMS

		Grade									
		5		6		7		8		Average	
		% Above	% Below	% Above	% Below	% Above	% Below	% Above	% Below	% Above	% Below
I	Sense of Pitch	11	89	13	87	10	90	16	84	12.5	87.5
II	Sense of Intensity	39	61	54	46	61	39	52	48	51.5	48.5
III	Sense of Time	39	61	38	62	51	49	39	61	29.3	70.7
IV	Sense of Rhythm	31	69	35	65	32	68	28	72	31.5	68.5
V	Sense of Consonance	48	52	52	48	40	60	34	66	43.5	56.5
VI	Tonal Memory	7	93	7	93	3	97	7	93	6.0	94.0
	Total av	34.1	65.9	33.2	66.8	32.8	67.2	29.3	70.7		

TABLE 4
COMPARISON OF MOUNTAIN CHILDREN WITH LARSON'S FINAL NORMS

	No	Med	Q-3	Q-1	Q
		Grade V			
Pitch					
Larson	1688	67 00±0.305	77.00	57 00	10 00
Mtn. child	123	51 41±0 869	60 56	45.15	7 71
Intensity					
Larson	1671	75.00±0 245	82 00	66 00	8 00
Mtn child.	123	69 08±1 039	79 42	60 98	9.22
Time					
Larson	1681	65 00±0 198	71 00	58.00	6 50
Mtn. child.	125	60 28±0 834	67.62	52.68	7 47
Consonance					
Larson	1680	62 00±0 193	68 00	55 00	6.50
Mtn. child	120	60.61±0 794	65 39	51 47	6 90
Tonal Memory					
Larson	1691	50.00±0 316	65 00	39.00	13 00
Mtn child	123	31 95±0 862	38.75	23 44	7.65
Rhythm					
Larson	448	64 00±0.296	69.00	59 00	5.00
Mtn child.	121	60 45±0.775	66.19	52.55	6 82
		Grade VIII			
Pitch					
Larson	1300	78 00±0 276	83 00	67 00	8 00
Mtn child.	91	57 95±1 611	69 07	44 42	12 30
Intensity					
Larson	1318	84 00±0 172	88 00	78 00	5.00
Mtn child.	91	81 88±1 334	92.70	72 34	10 18
Time					
Larson	1311	71 00±0.190	76 00	65 00	5.50
Mtn child	102	66 78±1 129	77 75	59 50	9 10
Consonance					
Larson	1315	67.00±0.190	72.00	61 00	5 50
Mtn child.	103	63 08±0.688	68 75	57 58	5.60
Tonal Memory					
Larson	1314	63 00±0 397	74 00	51 00	11.50
Mtn child	91	40 66±0 869	46 25	32.68	6 80
Rhythm					
Larson	587	69 00±0.258	74.00	64 00	5.00
Mtn child	90	62 22±1 241	71 56	52.75	9 40

no reliability measures, the comparisons of the two groups are statistically questionable

Studying the percentage of mountain children above and below the fiftieth percentile of Seashore's norms, the same general trend is shown (see Table 3)

Table 4 and Figure 1 show a comparison of the mountain children with Larson's (26) final norms for the Seashore Test.

Table 5 shows a comparison of the two groups in terms of medians and the reliability of the differences A study of this table shows that for Grade V there is a reliable difference in pitch, intensity, time, tonal memory, and rhythm For Grade VIII there is a reliable difference in pitch, time, consonance, tonal memory, and rhythm. These data indicate that the mountain children are below Larson's norms in all the measures, and that the differences are reliable in practically all comparisons There is a marked subnormality shown in pitch and tonal memory It has been interesting to speculate as to the cause of this It is, of course, a well-known fact that the mountain people do not have ready access to any instrument where the pitch is mechanically controlled The fiddle and dulcimer are tuned according to the individual player's sense of pitch. If we knew to what extent training influences accuracy of pitch, then we might speculate further on this point [8] There seems to be an evident relation between sense of pitch and tonal memory—lack of the first might impair the accuracy of the second as it is measured by the Seashore Test. This is suggested only as a possibility for the low achievement on these tests.

A study reported by Larson (26) in her revision of the Seashore norms is shown in Table 6 This study was made on eighth-grade students beginning music, and adult and eighth-grade non-musicians Here the mountain children appear superior to the beginning music group in all measures The greatest differences between the two groups are in intensity, time, and consonance (see Figure 2). The $\frac{D}{PE_{diff}}$ shows these differences to be reliable. We are fairly safe in saying that the mountain children are superior to this group of eighth-grade students Comparing the mountain children with the group of non-musicians, again the mountain children appear superior The difference is consistent and verified by the percentage of

[8] See Smith (45) for discussion

TABLE 5
MUSIC ABILITY OF MOUNTAIN CHILDREN COMPARED WITH LARSON'S FINAL NORMS

	No.	Med P.E	$\frac{D}{P.E._{diff}}$	Chances in 100
		Grade V		
Pitch				
Larson	1688	67 00±0 305		
Mtn child	123	51 41±0 869		
Difference		15 59±0 921	16 9	100
Intensity				
Larson	1671	75 00±0 245		
Mtn. child	123	69 08±1.039		
Difference		5 92±1 030	5 7	100
Time				
Larson	1681	65 00±0 198		
Mtn. child.	125	60 28±0 834		
Difference		4 72±0.851	5 5	100
Consonance				
Larson	1680	62 00±0 193		
Mtn. child	120	60.61±0 794		
Difference		1.39±0 817	1.7	87
Tonal Memory				
Larson	1691	50.00±0 316		
Mtn child.	123	31.95±0.862		
Difference		18 05±0.918	19 6	100
Rhythm				
Larson	448	64 00±0.296		
Mtn child	121	60 45±0 775		
Difference		3 55±0 830	4 2	100
		Grade VIII		
Pitch				
Larson	1300	78 00±0 276		
Mtn child.	91	57 95±1 611		
Difference		20 05±1.203	17 4	100
Intensity				
Larson	1318	84 00±0 172		
Mtn child	91	81 88±1 334		
Difference		2 12±1 341	1 58	85
Time				
Larson	1311	71 00±0 190		
Mtn child.	102	66 78±1.129		
Difference		4 22±1 146	3 69	99

TABLE 5 (*continued*)

	No	Med P E	$\frac{D}{PE_{Diff.}}$	Chances in 100
Consonance				
Larson	1315	67 00±0 190		
Mtn child	103	63 08±0 688		
Difference		3 92±0 714	5 4	100
Tonal Memory				
Larson	1314	63 00±0 397		
Mtn child.	91	40 66±0 869		
Difference		22.34±0 955	23.3	100
Rhythm				
Larson	587	69 00±0 258		
Mtn. child.	90	62.22±1 241		
Difference		6 78±1 268	5.3	100

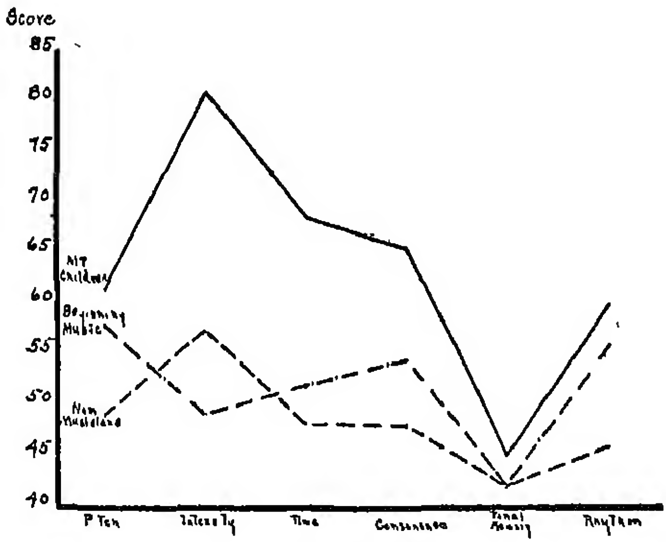

FIGURE 2

MUSICAL TALENT OF MOUNTAIN CHILDREN COMPARED WITH LARSON'S STUDY OF EIGHTH-GRADE BEGINNING MUSIC PUPILS AND WITH NON-MUSICIANS

TABLE 6

MOUNTAIN CHILDREN COMPARED WITH LARSON'S STUDY OF EIGHTH-GRADE BEGINNING MUSIC STUDENTS AND ADULT AND EIGHTH-GRADE NON-MUSICIANS

Eighth Grade, Beginning Music

	No.	Av P.E	$\frac{D}{P.E_{diff}}$	Chances in 100
Pitch				
Beginning music	121	57.6±2.810		
Mtn children	91	61 4±0.869		
Difference		—3 8±2.766	1.37	82
Intensity				
Beginning music	114	49 0±2.690		
Mtn children	91	80 6±1 039		
Difference		—31 6±2 884	10 96	100
Time				
Beginning music	119	51 7±2 600		
Mtn children	102	68 5±0 834		
Difference		—16 8±2.730	6.15	100
Consonance				
Beginning music	114	54 4±2 480		
Mtn children	103	65 5±0.794		
Difference		—11.1±2 604	4 26	100
Tonal Memory				
Beginning music	117	42.2±0 259		
Mtn. children	91	45 0±0.862		
Difference		—2 8±2 746	1 01	75
Rhythm				
Beginning music	112	55 6±0.258		
Mtn children	90	59 9±0.775		
Difference		—4 3±2.461	1.74	87

Adult and Eighth-Grade Non-Musicians

	No.	Av.	Percentage Above Non-musicians	Percentage Below Non-musicians
Pitch				
Non-musicians	100	48 9		
Mtn children	91	61.4	65	35
Difference		—12.5		
Intensity				
Non-musicians	100	57.0		
Mtn children	91	80 6	92	8
Difference		—23 6		

TABLE 6 (*continued*)

	No	Av	Percentage Above Non-musicians	Percentage Below Non-musicians
Time				
Non-musicians	100	48.0		
Mtn. children	102	68.5	88	12
Difference		—20 5		
Consonance				
Non-musicians	100	47 9		
Mtn children	103	65 5	92	8
Difference		—17 6		
Tonal Memory				
Non-musicians	100	42 1		
Mtn children	91	45 0	68	32
Difference		—2 9		
Rhythm				
Non-musicians	100	45 9		
Mtn children	90	59 9	80	20
Difference		—14 0		
Av %			80 6	19 4

overlapping. We are not able to give the true significance of this difference because Larson failed to report reliability measures with these data

Church, under the direction of Farnsworth (9), made a study of the results of the Seashore tests on the fifth and eighth grades in San José, California. Table 7 shows a comparison of the medians, also the percentage of mountain children above and below the California medians. There seems to be a fairly consistent difference in favor of the mountain children, but we are not able to say whether this difference is statistically significant since Church included no reliability measures.

A study made by W. S. Larson (27) reports the average performance on four groups of children in public-school music. He computed the average performance of each group on all of the six tests In Table 8 these groups are compared with the average performance of the mountain children We find the mountain children superior to the group beginning instrumental classes and to the members of the junior-high-school orchestra We could hardly expect

TABLE 7

MUSICAL TALENT OF MOUNTAIN CHILDREN COMPARED WITH THAT OF CHILDREN IN SAN JOSÉ, CALIFORNIA, MEASURED BY FARNSWORTH AND CHURCH

	Grade V			% mtn children		*Grade VIII*			% mtn children	
	Calif.	Mtn.	Diff.	Above	Below	Cal	Mtn	Diff.	Above	Below
Pitch	49	51	2	54	46	51	58	7	57	43
Intensity	48	69	21	93	7	46	82	36	96	4
Time	41	60	19	94	6	50	67	17	88	12
Consonance	50	61	11	65	35	49	63	14	92	8
Memory	41	32	9	11	89	59	41	18	11	89
Rhythm	51	60	9	66	34	24	62	38	100	0
Av %			11.8	63 9	36.1			21.7	75.7	24 3

our unselected group, which included no high-school grades, to surpass the junior-high advanced and senior-high orchestra. Since Larson did not report reliability measures, we are unable to give statistical reliabilities for these comparisons. The small number of cases in Larson's study also lowers the value of the comparison.

TABLE 8

AVERAGE PERFORMANCE OF MOUNTAIN CHILDREN ON ALL SIX SEASHORE TESTS COMPARED WITH AVERAGE PERFORMANCE AS MEASURED BY LARSON ON FOUR GROUPS OF CHILDREN IN SCHOOL MUSIC

	Groups I		II		III		IV		
	No	Av.	No	Av	No.	Av.	No.	Av.	Av Diff.
Larson	125	52.1	30	49 1	31	66 0	50	73.2	
Mtn child	426	59 3	426	59 3	426	59.3	426	59.3	
Diff		—7.2		—10 2		6.7		13.9	8

Group I, pupils beginning instrumental classes.
Group II, members of junior-high-school orchestra
Group III, members of junior-high-school advanced orchestra
Group IV, members of senior-high-school orchestra

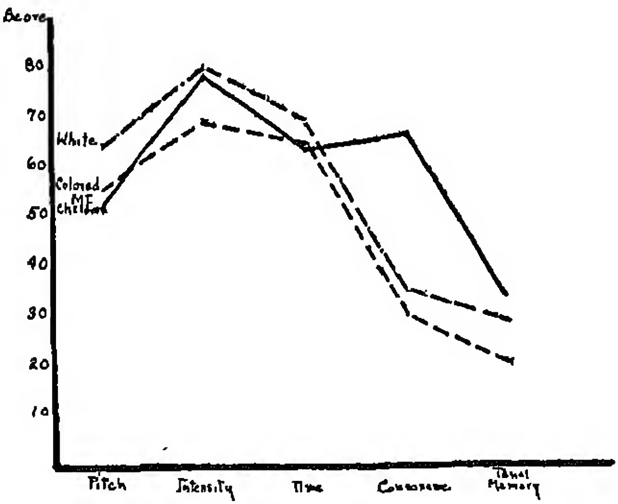

FIGURE 3

MUSICAL TALENT OF MOUNTAIN CHILDREN COMPARED WITH COLORED AND WHITE CHILDREN MEASURED BY GRAY AND BINGHAM

TABLE 9

Musical Talent of Mountain Children Compared with That of Colored and White Children as Measured by Gray and Bingham

	Pitch		Intensity		Time		Consonance		Memory		Average
	No.	Med.	No	Med	No	Med.	No	Med	No	Med.	
Colored child	258	56.1	258	69.2	258	65.7	258	32.5	258	21.2	
Mtn. child	419	53.2	419	78.6	446	64.2	456	62.3	410	34.4	
Difference		2.9		—9.4		1.5		—29.8		—13.2	—9.6
White child	219	64.7	219	80.1	219	70.2	219	31.9	219	29.4	
Mtn child	419	53.2	419	78.6	446	64.2	456	62.3	410	34.4	
Difference		11.5		1.5		6.0		—30.4		—5.0	—3.28

Various studies dealing with racial differences as measured by the Seashore Test have been reviewed by Farnsworth (8). He finds so many variations among the different studies that no definite conclusions can be drawn. This opinion is verified by other investigators. No attempt will be made in this investigation to discuss racial differences, but we would like to present several interesting comparisons of mountain children with Negro and Indian children

Table 9 gives a comparison of the mountain children with the colored and white children in Grades VI, VII, and VIII, studied by Gray and Bingham (15). Here the mountain children appear above the Negro in the tests on intensity, consonance, and tonal memory. The Negro children are above in tests in pitch and time, but with smaller differences The mountain children are below the white children of this study in pitch, intensity, and time. (See also Figure 3) Even without the reliability measures for these comparisons, the differences appear such that we can say that the mountain children compare favorably with the white children of Gray's investigation, and they are probably superior to the colored children in most of the measures. It is interesting to note in Gray's

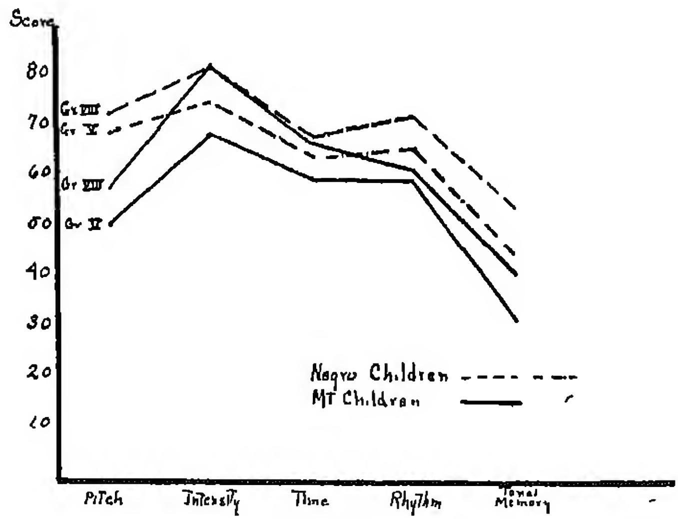

FIGURE 4

Musical Talent of Mountain Children Compared with That of Negro Children Measured by Johnson

TABLE 10

Musical Talent of Mountain Children Compared with That of 3350 Negro Children Measured by Johnson

	Grade V						Grade VIII					
					% mtn. children						% mtn children	
	Norm	Negro	Mtn.	Diff	Above	Below	Norm	Negro	Mtn.	Diff.	Above	Below
Pitch	67	69	51	18	11	89	77	73	58	15	19	81
Intensity	74	75	69	6	24	76	83	82	82	0	36	64
Time	64	64	60	4	30	70	71	68	67	1	41	59
Rhythm	65	66	60	6	12	88	70	72	62	10	19	81
Memory	50	45	32	13	7	93	65	54	41	13	15	85
Av		63.8	54.4	9.4	16.8	83.2		69.8	61.2	7.6	26	74

study the apparent discrepancy in the low scores for consonance and tonal memory.

Johnson (20) reports an investigation of the Seashore Test given to 3350 Negroes in North Carolina, South Carolina, and Virginia. A comparison of the mountain children with Johnson's data is shown in Table 10 and Figure 4. The Negroes appear superior to the mountain children in all measures except those of intensity and time in Grade VIII. The greatest differences appear in pitch and tonal memory. This is also shown by a study of the overlapping of the groups The indications are that the Negroes are probably above the mountain children in most of the measures, but as to the statistical significance we are unable to say The scores reported by Johnson in this study are above the Seashore-Larson norms in a number of measures. These are the highest scores we have found in any of the investigations It is unfortunate that we could not obtain reliability measures for Johnson's data in order to make accurate comparisons. We are inclined to believe that his data are rather high for Negro children

Garth (11) gave the Seashore Test of Musical Talent to 360 full-blood Indian children of Grades V-VIII in government schools in Oklahoma, South Dakota, and New Mexico A comparison of mountain children with these Indian children appears in Table 11 and Figure 5 Apparently from a study of the semi-interquartile range there is a great variation in the scores made by the Indian children This is also shown by the size of the P.E.'s. There seems to be a consistent difference in favor of the mountain children for most of the measures in all the grades. The $\dfrac{D}{P.E._{diff}}$ shows the differences to be reliable in all of the measures for Grade VIII, and in the majority of the measures in the other grades. It seems safe to say that the mountain children are superior to the Indian children in musical talent as measured by the Seashore Test We would like to mention that Garth's investigation is one of the best we have found for purposes of comparison because the investigator has included adequate data in terms of reliability measures As he reports that he found no racial differences, we did not use his data for mixed-blood Indians.

We have attempted to compare the musical talent of the mountain children as measured by the Seashore Test with the results of ten different investigations which used the same test. Seven of the com-

TABLE 11
COMPARISON OF MOUNTAIN CHILDREN WITH FULL-BLOOD INDIANS AS MEASURED BY GARTH

	No	Med	P.E	D / P.E diff	Chances in 100	No	Med	P.E.	D / P.E diff	Chances in 100
	Grade V					Grade VI				
Pitch										
Indians	83	24 5	3 192			45	44 7	4 934		
Mtn child	123	51 4	.869			123	51 4	869		
Diff.		—26 9	3 308	8 13	100		— 6.7	5 009	1.13	77
Intensity										
Indians	86	38 7	4.286			48	54 0	6 820		
Mtn child	123	68 1	1 039			123	69 1	1 039		
Diff.		—29 4	4 410	6 67	100		—15 1	6 898	2 19	93
Time										
Indians	77	73.1	4.259			48	78 5	5 535		
Mtn child	125	60 3	834			125	60 3	834		
Diff		12 8	4 339	2 95	97 5		18 2	5 593	3 25	98
Consonance										
Indians	65	51 8	5 147			46	63 3	4 091		
Mtn. child	120	60 6	.794			120	60.6	794		
Diff.		— 8 8	5 207	1 69	87		2 7	4 167	65	67
Memory										
Indians	71	16 0	3 406			41	9.2	3 314		
Mtn child	123	32 0	682			123	32 0	.862		
Diff		—16 0	3 513	4 55	100		—22 8	3 424	6 65	100
	Grade VII					Grade VIII				
Pitch										
Indians	73	25.1	1 909			74	20.9	2 775		
Mtn child.	96	53 9	1 037			91	58 0	1 611		
Diff.		—28 8	2 172	13.26	100		—37 1	3 208	11 56	100
Intensity										
Indians	77	33 7	4 573			73	27 3	3 544		
Mtn child.	96	82.4	.810			91	81.9	1 334		
Diff		—48 7	4 644	10 48	100		—54.6	3 786	14 42	100
Time										
Indians	79	66 9	4 766			64	44 3	4.216		
Mtn child.	117	69.2	919			102	66 8	1 129		
Diff		— 2 3	4 853	47	62		—22 5	4 364	5 15	100
Consonance										
Indians	69	44 5	4 438			74	44 3	3.255		
Mtn. child	94	69.0	1 065			103	63 1	688		
Diff.		—24 5	4 563	5 36	100		—18 8	3 326	5 65	100
Memory										
Indians	57	23.5	4 006			75	14 5	3 392		
Mtn child.	130	62 7	857			91	40 7	869		
Diff		—39 2	4 096	9 57	100		—26.2	3 501	7 48	100

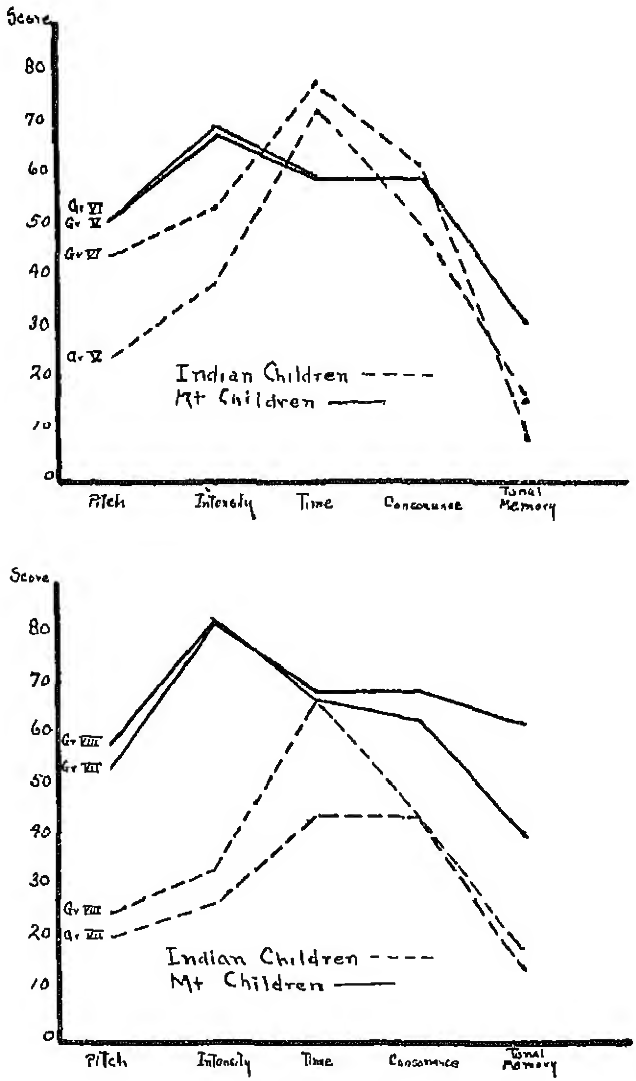

FIGURE 5

COMPARISON OF MUSICAL TALENT OF MOUNTAIN CHILDREN WITH THAT OF FULL-BLOOD INDIAN CHILDREN MEASURED BY GARTH

parisons had to be made without adequate reliability measures. Of these seven comparisons, the mountain children seem to fall below in two, above in four, and to show no difference in one. Of course these comparisons are questionable as to reliability. Of the three comparisons made with adequate reliability measures the mountain children fall below in one and above in two. We feel fairly safe in saying that the mountain children compare favorably in musical talent with the normal for the country at large.

Summary and Conclusions

1. This investigation consists of a study of the musical talent of 465 East Tennessee mountain children measured by the Seashore Test of Musical Talent

2 The mountain children seem to fall slightly below the Seashore norms in practically all the measures, and significantly below in pitch and tonal memory.

3. In comparing the mountain children with Larson's revision of the Seashore norms, the mountain children fall below the norms in practically all measures, and a reliable statistical difference is found between the groups.

4. There is a significant difference in favor of the mountain children when compared with Larson's eighth-grade beginning music, and there seems to be a real difference in favor of the mountain children when compared with her study of adult and eighth-grade non-musicians.

5. Mountain children seem superior to children of San José, California, in most of the measures of the Seashore Test.

6. Mountain children seem to be above W S Larson's group of beginning instrumental classes in public-school music and above his junior-high-school orchestra group.

7 Mountain children are above the white and Negro children as reported by Gray and Bingham.

8 The Negro children as reported by Johnson seem to be superior to the mountain children in practically all measures of the test.

9. There is a significant difference in favor of the mountain children when compared with full-blood Indian children measured by Garth.

10. The musical talent of East Tennessee mountain children, as measured by the Seashore Tests, compares favorably with that of other unselected groups in various sections of the country.

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East Tennessee State Teachers College
Johnson City, Tennessee

L'APTITUDE MUSICALE DES ENFANTS MONTAGNARDS D'EAST TENNESSEE

(Résumé)

Les montagnards des Appalachiens du Sud sont uniques à l'egard de leur capacité à préserver oralement les romances de la mère patrie de génération en génération. Pour étudier cette capacité et pour mesurer l'aptitude musicale en jeu on a fait subir le Test Seashore d'Aptitude Musicale à 465 écoliers des "grades" V-VIII des ecoles montagnardes d'East Tennessee. On ne peut dire à quel degré le test Seashore mesure l'aptitude innée ou à quel point le milieu l'influence, mais les résultats offrent des données pour des comparaisons de la hauteur, de l'intensite, du temps, de la consonance, de la mémoire tonale, et du rythme

On a fait des comparaisons avec des groupes dans diverses sections du pays *Les enfants montagnards semblent un peu inférieurs aux standards* Seashore dans la plupart des mesures et superieurs aux enfants en Californie, Texas, et certaines sections d'Iowa Les enfants negres de Texas sont inférieurs aux enfants montagnards tandis que les nègres de la Caroline du Nord leur sont supérieurs Les enfants montagnards sont superieurs aux Indiens des écoles du gouvernement Dans trois comparaisons des

enfants montagnards et d'autres groupes faites avec des mesures adéquates de constance, les enfants montagnards sont supérieurs en deux et inférieurs dans l'autre Dans quatre comparaisons sans mesures de constance, les enfants montagnards sont supérieurs en trois Quand on considère toutes les comparaisons, il paraît que les enfants montagnards comparent favorablement avec d'autres groupes dans l'aptitude musicale comme la mesure le Test Seashore.

WHEELER ET WHEELER

DIE MUSIKALISCHE BEGABUNG DER KINDER DER GEBIRGE DES ÖSTLICHEN TEILES DES STAATES TENNESSEE

(Referat)

Die Bergbewohner der sudlichen Appalachianen sind in der Fahigkeit ohne Gleichen, die alten Balladen des einstigen Vaterlandes Generationen hindurch mundlich lebendig zu erhalten. Um diese Fahigkeit zu untersuchen, und um die darin mit einbegriffene musikalische Begabung zu messen, pruften die Verfasser mit dem Seashore Test der musikalischen Begabung 465 Kinder aus der V-VIII Schulklasse in Schulen der Gebirge des ostlichen Tennessee Wir sind nicht im Stande, zu sagen, bis zu welchem Grade der Seashore Test die angeborene Begabung messt, oder wie stark die Testleistungen durch die Umgebung beeinflusst werden, aber die Befunde liefern Daten, die Vergleichungen in Bezug auf Tonhohe, Tempo, Konsonanz, Tongedachtnis, und Rhythmus ermoglichen

Es wurden Vergleichungen ausgefuhrt mit Gruppen aus verschiedenen Gegenden der Vereinigten Staaten. Die Gebirgskinder fielen in Bezug auf die Mehrzahl der gemessenen musikalischen Teilfahigkeiten unter die Seashore'schen Testnormen, waren aber Kindern in Kalifornien, Texas, und gewissen Teilen Iowas uberlegen Negerkinder in Texas stehen tiefer, als die Gebirgskinder, wahrend Negerkinder aus North Carolina hoher zu stehen scheinen Die Gebirgskinder sind Indianerkindern aus den Regierungsschulen uberlegen. Aus drei Vergleichungen, in denen die Messungen genugende Zuverlassigkeitszahlen lieferten, zeigen sich die Gebirgskinder in zwei uberlegen und in einer tieferstehend. Aus vier Vergleichungen ohne Zuverlassigkeitszahlen [reliability measures] zeigten sich die Gebirgskinder in drei uberlegen Unter Berucksichtigung aller Vergleichungen scheint es, dass Gebirgskinder anderen Gruppen in Bezug auf musikalische Begabung, mit dem Seashore Test gemessen, im Allgemeinen uberlegen sind

WHEELER UND WHEELER

A STUDY IN THE PREDICTION OF MOTOR RHYTHMIC PERFORMANCE OF SCHOOL CHILDREN*

From the Iowa Child Welfare Research Station, The State University of Iowa

HAROLD M WILLIAMS

One of the fundamental problems in the evaluation of tests which show a gain with increasing age is the measurement of their predictive efficiency The results of a repetition of the Sievers (8) motor rhythm test on 100 elementary-school children after a 15-month interval are presented here as a preliminary contribution to this problem in the field of the musical abilities [1]

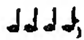
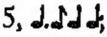
RESULTS OF RETEST ON MOTOR RHYTHMIC PERFORMANCE

The technique of the Sievers test for motor rhythm was based on that of Seashore (6). In this test a mechanically produced pattern was repeated continuously for a given number of times, the subject reproducing it simultaneously. A graphic record of performance was obtained. Sievers' modification consisted in the use of a series of patterns graded in difficulty. The score was the summation of points earned by the child in a series of trials of predetermined length, score points being allowed only for responses falling within a fixed margin of error. Sievers used five patterns as follows.

These were given twice in double fatigue order for the complete test Ten consecutive repetitions of the pattern were counted for each trial Under these conditions the test was adequate for a range of ability from that of the four-year-old child to that of the university music student The predicted reliability for the complete test was .911 over the age range of six to twelve years.

*Recommended by George D Stoddard, accepted for publication by Carl Murchison of the Editorial Board, and received in the Editorial Office, December 5, 1932

[1]These children were made available for retest through the courtesy of those in charge of the St Patrick's School of Iowa City, Iowa, to whom grateful acknowledgment is hereby made

In the retest the procedure of Sievers was repeated as exactly as was possible within the administrative necessities. The general physical conditions and the instructions were the same. A similar fore-exercise was given in each test. The same response key was used [the rhythm hammer as previously described (8)]. A counter device was used to record the score. This yielded results which are identical with the cumulative method of scoring used by Sievers when the margin of error allowed is the same. The margin of error was ±.05 second in both tests. Four of the sequence of patterns finally recommended by Sievers (Patterns 1, , 3, ; 5, ; and 7, ) were used in the retest.

The following minor changes were made. Since previous work had shown that the auditory cue was the critical one in the stimulus, the patterns were sounded out by a small electromagnet placed before the child and the visual cue given by the Hollerith counter was omitted. As a check on the flexibility possible in the test, one new pattern consisting of a simple equal periodicity of 067 second () was substituted for Sievers' Pattern 2 ().

When the score in this test is recorded by the counter method, it is possible to achieve a certain score by random tapping. A correction for chance success may be calculated by making two assumptions: (1) that in random tapping the strokes tend to distribute themselves equally over the whole circumference of the drum, and (2) that a certain maximum rate of tapping is maintained. The second assumption is necessary because the slower the tapping the smaller will be the chance score. On the basis of these assumptions the correction is made by subtracting from the child's score a constant determined by the formula $T—(T \text{x} \frac{L}{C})$ (T is the total possible score, L is the total length of metallic contacts in a given pattern, and C is the circumference of the drum.) In our observation neither of the assumptions was adequately realized in the test situation. The first assumption requires a long sample to attain

stability On the whole, the children who failed did not tap as rapidly as the full correction demanded An empirical check showed that the formula overcorrected in many cases The scores used for computation in both the test and retest were, therefore, the uncorrected values

Since the time allowed for the retest did not permit of repeating the sequence in the double fatigue order, only the series in ascending order of difficulty was used The scores from the comparable part of the Sievers data were used for the first test scores The reliability of one-half of the Sievers test, as determined by the correlation between halves, was approximately .85

The data for this study, therefore, consist of the summed scores on (1) the original Sievers series, (2) the complete retest series, and (3) the four patterns common to both series The total possible scores were. Sievers original series, 210; complete retest series, 190, series common to both, 170 Comparisons are made on both the identical series and the dissimilar series

The age and grade distribution of the children at the time of the original test are shown in Table 1 (age six representing the range

TABLE 1

								Total
Age	6	7	8	9	10	11	12	
N	12	4	15	17	24	19	9	100
Grade	1	2	3	4	5	6	7	
N	9	9	9	17	20	19	17	100

from five and one-half to six and one-half years, and so on). The retest group was limited to those children who had made normal school progress during the year. The distribution of scores on the identical series is given in Table 2 The distribution of scores on the dissimilar series is given in Table 3

It will be seen from Tables 1, 2, and 3 (1) that there was a wide range of scores on each test at each age level; (2) that the mean values increased with age over this range, (3) that there was a tendency for age differences in score to decrease with age; and (4) that there was an increase in mean raw scores over the 15-month period, this gain being greatest at the lower age levels

The predictive power of the test was evaluated in several ways. Correlations were computed for the total group for both the identical

TABLE 2

Age at first test	N	Sievers test M	σ	Range	Retest M	σ	Range
6- 8	31	32.8	25.6	7- 97	45.6	16.4	18- 98
9-10	41	60.9	36.3	7-134	67.7	23.3	31-138
11-12	28	81.7	40.5	6-136	81.1	26.8	30-145
Total group	100	58.5	40.9	6-136	66.9	27.5	18-145
Grade at first test	N	M	σ	Range	M	σ	Range
1-3	27	31.2	23.3	7- 81	42.7	12.6	18- 80
4-5	37	57.8	40.3	7-134	72.5	24.1	31-138
6-7	36	80.0	41.4	6-136	83.2	27.1	30-145
Total group	100	58.5	40.9	6-136	66.9	27.5	18-145

TABLE 3

Age at first test	N	Sievers test M	σ	Range	Retest M	σ	Range
6-8	31	50.8	26.5	8-147	52.2	13.4	21-100
9-10	41	91.1	50.5	7-183	81.6	19.5	35-151
11-12	28	116.9	52.8	9-186	92.7	27.5	32-161
Total group	100	84.8	55.1	7-186	75.0	29.7	21-161
Grade at first test	N	M	σ	Range	M	σ	Range
1-3	27	49.4	34.3	8-147	49.0	15.6	21- 69
4-5	37	86.1	56.1	7-183	80.8	25.3	35-151
6-7	36	109.0	55.1	9-186	88.5	29.5	32-161
Total group	100	84.8	55.1	7-186	75.0	29.7	21-161

and dissimilar series. To eliminate as far as possible the effect of age on the correlations, similar computations were made for the three age and grade subgroups. As a further check the partial-correlation technique was used with the whole group, the Sievers test being designated by the subscript 1, the retest by 2, and chronological age by 3. The raw correlations between the test and retest (r_{12}) for the identical series are given in Table 4. The raw correlations for the dissimilar series were as shown in Table 5.

TABLE 4

		r_{12}	PE_r	N
Total group		.742	.030	100
Ages	6- 8	.731	.055	32
Ages	9-10	.719	049	40
Ages	10-11	.683	068	28
Grades	1- 3	.889	026	28
Grades	4- 5	704	056	37
Grades	6- 7	761	046	35

TABLE 5

		r_{12}	PE_r	N
Total group		707	033	100
Ages	6- 8	402	102	31
Ages	9-10	632	063	41
Ages	10-11	743	058	28
Grades	1- 3	497	.094	27
Grades	4- 5	633	066	37
Grades	6- 7	688	055	36

In the partial-correlation analysis the coefficients given in Table 6 were obtained from the identical series. The correlation between initial scores and gains in the identical series was .728±.032. In the dissimilar series the coefficients were as shown in Table 7.

TABLE 6

r_{12}	742
r_{13}	.306
r_{23}	483
$r_{12.3}$	713

TABLE 7

r_{12}	707
r_{13}	486
r_{23}	.528
$r_{12.3}$	.606

The following conclusions are drawn from the data presented: (1) The reliability (predictive power) of this test when repeated after a 15-month interval on 100 children ranging in age from six to thirteen years was .74, as compared with a reliability of 85 at

a single sitting. The use of the more reliable complete form of the test would probably have resulted in a still better prediction. (2) In general, the coefficients for the identical series were higher than those for the dissimilar series, suggesting that the highest prediction can be obtained only with a carefully standardized procedure (3) Eliminating the influence of heterogeneity in age reduced the correlations only slightly in the identical series. There is a suggestion that the effect of age heterogeneity is greater in the dissimilar series.

Historically, the present test procedure belongs in the sequence of studies by Sears (5), Révész (4), Prager (3), Vance and Grandprey (7), Seashore (6), Nielsen (2), Henderson (1), Williams (8), and Sievers (8) on regulated motor rhythm Following the plan of the last two authors named, a scoring system was employed in which the attempt was made to combine two types of criteria used by preceding investigations into a single figure. The two types of criteria were the level of complexity of rhythmic pattern reached by the child and the accuracy with which he was able to tap these rhythms This was done by cumulating the accuracy scores over a sequence of patterns varied in difficulty. In the writer's opinion this method gives one, but by no means the only, significant general index of accomplishment in rhythmic control At present this and all other measures are in particular need of validation against performance in functional musical situations

The particular evidence sought for in the present study was the degree to which this type of test is capable of predicting the future performance of children under normally stable conditions Within the limits of the experiment, the test results indicate only a moderate falling-off in reliability over a 15-month interval when identical test elements were used The need for further study over longer periods of time is obvious. Results from consecutive measurements on the same children in this study agree with the results of Sievers (8) and Seashore (6) from group comparisons indicating a growth in score with age They parallel also the results of Sievers in showing a negative acceleration in raw score gains at the upper range of ages considered. In both respects the findings of this study agree with the group results of Sears (5) and Prager (3), although in those two studies the criterion was the level of complexity reached by the child.

New Apparatus for Tests of Motor Rhythm

The usefulness of a sequence of patterns for testing motor rhythmic performance over a wide age range has been pointed out by Sievers (8). A device which will permit of the rapid presentation of a wide variety of rhythmic patterns may also be of value in many other types of studies, for example, in studies of transfer of training in this motor function and so on. The chief requirements in apparatus are precision, flexibility, and rapid scoring. Two new types of apparatus fulfilling in part, at least, these requirements are described here. The first is a modification of the Seashore (6) motor rhythm apparatus with counter scoring, the second an adaptation of his graphic method allowing for a rapid change of pattern. Both devices are designed so as to use as far as possible standard laboratory units. The counter apparatus requires only the pattern drum as a special

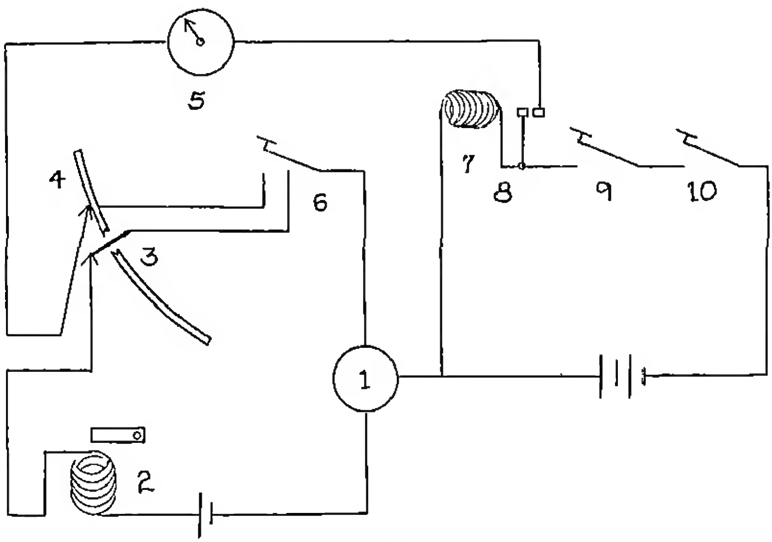


FIGURE 1A

The Electrical Circuit

1—ground, *2*—stimulus, *3*—stimulus contact, *4*—response contact, *5*—impulse counter, *6*—selector switch, *7*—relay solenoid, *8*—relay circuit breaker, *9*—subject's key, *10*—experimenter's switch

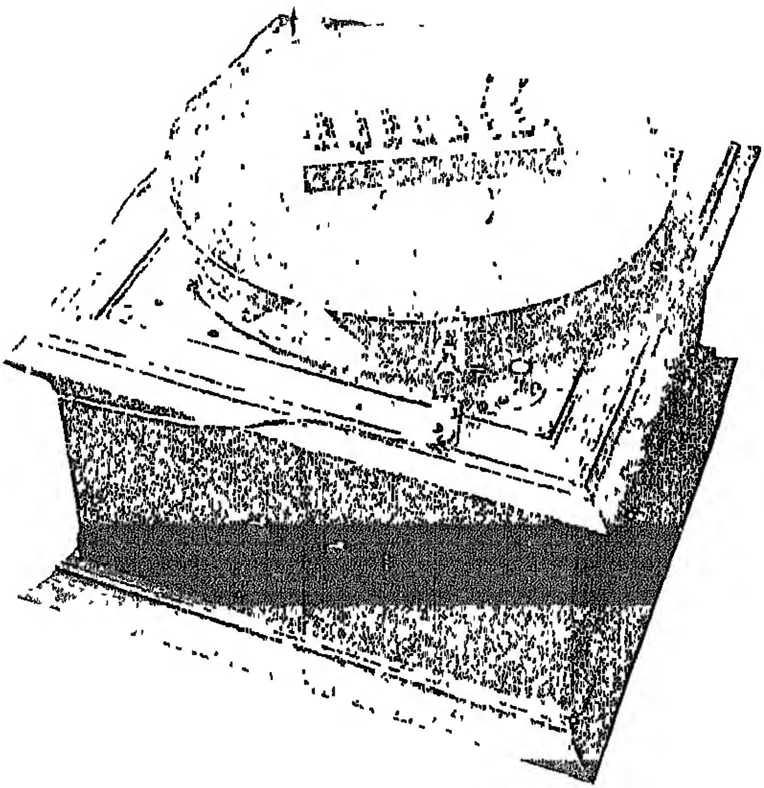


FIGURE 1B
THE PATTERN DRUM AND BRUSHES

unit The graphic device calls for a special pattern disc and the Seashore pen recorder.[2]

The device used in this study adopted the counter method of scor-

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[2]Obtained from C. H. Stoelting, Chicago, Illinois

ing, the pattern being varied by a set of switches The electrical circuit and the finished pattern plate are shown in Figure 1. The pattern plate consists of a built-up walnut drum 1¼ inches high and 12 inches in diameter. At the center a metal bushing fits accurately on the motor shaft furnishing the ground return connection through the motor. The bottom of the drum is slightly hollowed to accommodate the wiring Around the periphery are inlaid eight brass response contacts. One of these is used as a base and the other contacts are located the following degrees from it, counting counterclockwise: 90, 120, 135, 180, 240, 270, and 300 degrees. The contacts are as shown in Figure 1B. Various margins of error of ±.05, 04, .03, and 02 seconds are etched on the surface. A brass standard screwed to the phonograph frame carries a phosphor-bronze double-spring contact in a slot and a set-screw arrangement which permits making contact at any desired error value

The stimulus contacts are metal pins insulated from the response contacts, and set sufficiently in advance from the centers of the response contacts to allow for magnet latencies The stimulus circuit is completed by a separate insulated spring on the standard. This arrangement makes it possible for each contact unit to be completely eliminated from the circuit by a single three-point switch In the apparatus of Figure 1B seven small knife switches were used, one for each variable contact These were mounted compactly on the top of the drum The method requires that the experimenter switch the response key in and out for a predetermined number of revolutions By this arrangement well over thirty common musical patterns in various combinations of double and triple time may be employed by merely closing the proper group of switches The limits of accuracy allowed for a successful score point vary from ± 02 second to ±.05 second by setting of the brush contact Suitable arrangements of spring strength and damping (with felt strips) render this part of the apparatus quite inaudible.

For a counter the Cenco impulse counter[3] is very rapid and efficient. No zero setting is required as the cumulative score can be recorded in terms of the figures at the beginning and the end of a trial. A standard telegraph relay, arranged so that the movement of the armature breaks the counter circuit when the response key is depressed, forms a satisfactory relay Its use has already been described by Seashore (6).

[3]Manufactured by Central Scientific Company, Chicago, Illinois

The response key may be a telegraph key or the Williams (8) rhythm hammer, elsewhere described It is advisable to inclose the counter and relay in a sound-proof box. The advantages of the apparatus are relative economy in cost, the rapid change of stimulus pattern, and the time economy of the counter method of scoring Its limitations are (1) a prearranged number of revolutions per trial must be completed by the subject, (2) the experimenter must switch these in and out at the proper time, and (3) the data are not analytical.

Where it is desired to record the responses in detail, a special plate has been designed. This is a 12 5-inch heavy metal disc fitting on the phonograph turntable On the outer one-half inch of the plate stimulus pegs are fastened with screw bolts and stops so that the points for a given pattern may be swung outward to close the circuit with the usual spring contact, all other points can be swung inward parallel with the outer edge of the plate. The record paper is placed on the disc, and the whole clamped down with a screw inserted in the motor shaft. The Seashore pen response device completes the necessary special apparatus It is possible to increase further the flexibility of this apparatus by inserting the stimulus point supports in slots concentric with the periphery of the disc This makes possible studies of the response to aesthetic variations in the temporal relations of rhythmic patterns. In the writer's opinion this is the best apparatus at present from the point of view of simplicity of operation and completeness of the record.

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Iowa Child Welfare Research Station
The State University of Iowa
Iowa City, Iowa

UNE ÉTUDE DE LA PRÉDICTION DE L'EXÉCUTION MOTRICE RYTHMIQUE DES ÉCOLIERS

(Résumé)

Cent écoliers de l'école élémentaire, âgés de six à douze ans, ont subi de nouveau le test du rythme moteur de Sievers après un intervalle de quinze mois Ce test mesure la précision avec laquelle on frappe une série de petits coups en formes rythmiques avec une reproduction simultanée Un résultat cumulatif, basé sur le nombre des coups qui tombent dans une limite fixe d'erreurs dans un nombre fixe de répétitions, est le critère. Dans le nouveau test on n'a employé chaque forme qu'une fois, au lieu de l'employer deux fois en ordre de fatigue double comme dans le test entier. On a ajouté aussi une forme nouvelle, ce qui a rendu possible la comparaison des résultats avec une série identique et une série dissemblable. On a obtenu les résultats suivants (1) Les différences individuelles ont été très marquées dans les résultats (2) Les comparaisons entre les groupes et celles entre le premiers tests et les seconds tests ont montré un accroissement du résultat brut avec l'âge, la courbe d'accroissement suggérant une accéleration négative (3) Tandis que la constance du test a été de 0,85 la première fois, la constance après un intervalle de quinze mois a été de 0,742 dans la série identique et de 0,707 dans la série dissemblable. (4) L'analyse partielle des corrélations a montré peu de décroissement de ces valeurs dû à l'hétérogénéité de l'âge. On conclut qu'une répétition qui emploie des éléments identiques dans la forme complète du test devrait montrer une persistance assez prononcés des différences individuelles On décrit des appareils nouveaux qui ont facilité l'administration du test

WILLIAMS

EINE UNTERSUCHUNG DER VORAUSSAGBARKEIT MOTORISCH-RHYTHMISCHER FAHIGKEIT BEI SCHULKINDERN

(Referat)

Hundert Elementarschulkinder, von sechs bis zwolf Jahre alt, wurden nach Verlauf von 15 Monaten mit dem Siever'schen Test zur Prufung des motorischen Rhythmus [Sievers' test of motor rhythm] zum zweiten Mal gepruft Mit diesem Test wird die Genauigkeit gepruft, mit der eine Serie rhythmischer Gestaltungen [rhythmic patterns] bei simultaner Darstellung (production) durch Klopfen markiert wird [is tapped in simul-

taneous reproduction] Als Kriterium dient eine angehaufte Zahl [cumulative score], die auf die Zahl der innerhalb einer bestimmten Fehlergrosse [margin of error] bei einer bestimmten Frequenz der Wiederholungen liegenden Klopfungen [taps] basiert ist Bei der Wiederprufung wurde jede Gestalt bloss einmal verwendet, anstatt zweimal in einer Anordnung, welche die Ermudung verdoppelte [double fatigue order], wie bei dem vollstandigen Test Es wurde auch eine neue Gestalt hinzugefugt, welche die Vergleichung der an einer "identischen" mit den an einer "nicht-ahnlichen" [dissimilar] Serie erhaltenen Zahlen ermoglichte. Es wurden folgende Resultate ermittelt. (1) Die Unterschiede zwischen den durch verschiedene Versuchspersonen erzielten Zahlen waren stark ausgepragt (2) Vergleichungen sowohl zwischen verschiedenen Gruppen wie auch zwischen der ursprunglichen Prufung und der Wiederprufung offenbarten eine mit zunehmendem Alter einhergehende Zunahme der erzielten absoluten Zahl [raw score], wobei die Gewinnskurve [curve of gain] auf negativ Beschleunigung [negative acceleration] hinwies (3) Während die Zuverlassigkeitsziffer [reliability] des Tests mit einer Sitzung 85 betrug, betrug sie nach Verlauf von 15 Monaten in den identischen Serien 742 und in den nicht-ahnlichen Serien 707 Eine Analyse mit Teilkorrelationen wies auf nur geringe Abnahme dieser Zahlen in Folge der Altersverschiedenheiten hin Es wird die Schlussfolgerung gezogen, dass eine Wiederholung unter Verwendung identischer Elemente in der vollendenten Form des Tests eine betrachtliche Beharrung der individuellen Unterschiede offenbaren durfte Ein neues Apparat zur Erleichterung der Ausfuhrung des Tests wird beschrieben

WILLIAMS

A STUDY OF THE RELATION OF AGE INTERVAL TO DEGREE OF RESEMBLANCE OF SIBLINGS IN INTELLIGENCE*[1]

From the College of Education and Institute of Child Welfare, University of Minnesota

F H. FINCH

INTRODUCTION

1. *The Problem* The problem of this investigation is to determine, within the limits to which it is possible at present to measure intelligence, whether the degree of resemblance in intelligence in pairs of children born to the same parents is related to the interval of time separating the birth of the children It seems reasonable to assume that there are, on the average, more differences in the home conditions and experiences in general of siblings separated in age by several years than of siblings separated by a shorter interval of time The question here to be considered is whether such differences in surroundings affect the degree of resemblance of siblings to an extent that will be reflected in test results

2 *Related Studies* Studies of family resemblance date from the classic work of Galton (3), and numerous papers on the subject have since come from the Galton Laboratory.

Pearson (15, 17) followed Galton with two papers in which he reported the results of investigations based on data from several thousand individuals Correlations for both physical and mental traits of siblings were found to approximate .50

Heron (7) has examined the records of 331 families, from which he concluded that fraternal resemblance in insanity is expressed by coefficients ranging between 45 and 55.

Schuster and Elderton (18) published in 1907 a study of the

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[1]From a thesis submitted to the Graduate Faculty of the University of Minnesota in partial fulfillment of the requirements for the degree of Doctor of Philosophy, done under the direction of Professor W S. Miller Thanks are also due the public schools of St Paul, Minnesota, and St Louis, Missouri, from whose records certain of the data were obtained; and to Professor M J Van Wagenen, Mr Claude L Nemzek, and Louise Asbury Finch, for their suggestions and assistance.

resemblance of the records of brothers found in the class lists of Oxford and in the school lists of Harrow and Charterhouse. The values of r as a measure of resemblance were .405 and .398, respectively. Pearson, in an appendix to the above paper, pointed out that these values are affected by the restricted range of the data, and suggested that the true value lies between .5 and .6.

Pearson (16) found that when the members of 2801 pairs of siblings were classified by teachers into six categories representing different degrees of general intelligence the correlation measuring the resemblance was .515. This agrees surprisingly well with his earlier figure for resemblance of siblings in physical traits (15, 17).

Gordon (4) reported the correlation between intelligence quotients for 216 pairs of siblings (432 individuals) tested in a California orphanage as .61. These data have been reworked by Elderton (2) who found that for all possible pairs this value became .544 when the results were corrected for variation in age at the time of testing.

Hildreth (8) examined the degree of resemblance in intelligence of siblings from three sources. The composite group of 1028 pairs gave a coefficient of correlation of .68, which, when corrected for variation in age, became .419. She also reported a resemblance of .23 among 78 pairs of sibs reared apart, which, when corrected for curtailed range, gave .495.

Hart (6) examined the degree of resemblance in intelligence of sibs in three different school systems. Coefficients for the three groups were .477±.034, .459±.066, and .399±.057.

Jones (10) reported a coefficient of resemblance of .49 for 828 pairs of siblings from families representative of a rural New England community. He employed the Stanford Revision below age 14, and Army Alpha above that age, and combined the results by converting into sigma indices.

Willoughby (25) administered a battery of verbal and non-verbal tests to 141 families. The average coefficient of sibling resemblance for 11 tests was .43.

Thorndike (23) reported coefficients of resemblance of .40±.026, .66±.018, and .73 (N=823) for groups of siblings attending high schools. The subjects were tested with the I.E.R. tests, which have a relatively high reliability. From this, after correcting for at-

tenuation in the test results and allowing for selection among the subjects, Thorndike estimates the resemblance to be 60

While there appears some disagreement among the findings of these investigators, probably due in large measure to different degrees of selection in data from various sources, and to the use of test results of varying reliability, it seems reasonable to consider the coefficient of sibling resemblance in both physical and mental characters to be near .50, or perhaps slightly higher.

Investigations bearing directly on the relation between sibling resemblance and age interval are few in number and, as will be seen from the following, much less conclusive than those dealing with sibling resemblance in general.

The two earlier papers, those by Starch (20) and Griffitts (5), compare resemblance of adjacent and non-adjacent siblings in school achievement as measured by teachers' marks Starch, using a composite of marks for one year, found the resemblance, expressed in terms of rank correlation, to be as follows.

	ρ	N
First and second child	58	63
Second and third child	64	24
First and third child	34	24

Griffitts' investigation is similar in nature, but is based on more extensive data, including teachers' marks for children from 657 families. The median of all marks given a child during a three-year period has been employed as a measure of achievement The following correlations are given as measures of resemblance:

	r	N
First and second child	.384	657
Second and third child	332	226
Third and fourth child	300	89
Fourth and fifth child	270	29
First and third child	279	226
Second and fourth child	293	89
Third and fifth child	690	29
First and fourth child	208	89
Second and fifth child	640	29
First and fifth child	.510	29

It will be noted that the high values for widely separated pairs are based on very small numbers, and it is apparently the opinion of the author that the actual resemblance for non-adjacent pairs is

the smaller. However, if one takes into consideration the fact that, for the two members of pairs farthest apart, marks were probably given under conditions least alike, there is obviously no evidence adequate for any very definite conclusions.

There are also two published studies employing intelligence test results that must be considered. Tallman (22), in her study of twins, incidentally mentioned the fact that among 199 children (not twins) from 72 families tested with the Stanford Revison of the Binet scale she observed in all possible pairs of siblings an average IQ difference amounting to 13.14± 709, whereas the average difference for the 64 pairs less than two years apart in age was 11.96± 963. No correction was made for the variation in age at the time the tests were given Jones and Hsiao (11), in their investigation of the relation of birth order to intelligence, found the mean superiority of younger over older sibs to be 010 σ for 330 adjacent pairs, compared with a superiority of .055 σ for 284 non-adjacent pairs. For the same data the median difference was 035 σ in favor of the older among the adjacent, and .058 σ in favor of the younger among the non-adjacent pairs.

The one exhaustive study of the relation of age difference to sibling resemblance is found in the work of Stocks (21) in which he examined resemblance in ten physical and physiological characteristics. As the subjects ranged from 3 to 15 years in age at the time of measurement, corrections for age were made in all cases. In the aggregate of the ten characters the results were as shown in Table 1.

TABLE 1

Age diff. in months→		9 to 33	33 to 69	69 to 129
Opposite sexed pairs	N	716	899	349
	M_{diff}	.9232±.0238	8695±.0242	9155±.0407
Like sexed pairs	N	624	807	306
	M_{diff}	8492± 0284	.8881± 0261	8413± 0505

For the characters in question, the author is obviously justified in his conclusion that "there is no definite evidence that the interval between births has any influence on degree of resemblance . . ." (p. 107).

The Data

The data for the present study consist of intelligence test records for 1023 pairs of native-born white siblings coming from 614 families,

and including 1401 individuals (See Table 2) These data, obtained from three sources, (1) University High School, University of Minnesota, (2) Mason and Grant public elementary schools, St Louis, Missouri, and (3) the Special Class Offices of the public schools of St Paul, Minnesota, furnish samples of superior, average, and inferior test ability, respectively (See Table 3)

The University High School group includes 359 pairs from 239

TABLE 2
DISTRIBUTION OF SIBLINGS BY FAMILIES*

No of cases from family	No of families	No of pairs	No of cases
	Total data		
6	4	60	24
5	7	70	35
4	24	142	96
3	88	260	264
2	491	491	982
Totals	614	1023	1401
	University High School data		
5	1	10	5
4	9	54	36
3	33	99	99
2	196	196	392
Totals	239	359	532
	St. Louis data		
4	4	24	16
3	17	50†	51
2	125	125	250
Totals	146	199	317
	St Paul data		
6	4	60	24
5	6	60	30
4	11	64‡	44
3	38	111§	114
2	170	170	340
Totals	229	465	552

*It should be noted that number of cases from a family means only the number of cases for which usable records were available, and not total family

†One twin pair excluded.

‡Two twin pairs excluded

§Three twin pairs excluded

TABLE 3

INTELLIGENCE OF SIBLINGS

		Mean	P.E. of mean Max	P.E. of mean Min	σ	N
U High data	Older sib	117 6	55	47	13 15	359
	Younger sib	118.0	47	.43	12 00	359
St. Louis data	Older sib	98 6	.70	61	12 75	199
	Younger sib	103.8	75	.66	13.75	199
St. Paul data	Older sib	76 8	41	.33	10 90	465
	Younger sib	81 9	43	34	10.50	465

families, and is made up of 532 individuals Of these, the members of 278 pairs were tested with five group tests, (1) Army Alpha, Form 8, (2) Pressey Senior Classification, (3) Haggerty Delta 2, (4) Terman Group Test of Mental Ability, Form A, and (5) Miller Mental Ability Test, Form A Intelligence quotients were computed for each individual from the results of each group test, according to the instructions furnished in the authors' manuals accompanying the various tests. The quotients from those tests other than the Terman were next converted to the equivalent of Terman Group intelligence quotients by the method employed by Miller (14). Of the five values thus obtained for each individual, the median has been taken as the best available measure. The remaining 81 pairs were made up of cases tested with the Stanford Revision of the Binet Scale

The University High School data have been drawn from the records of children entering the school between 1919 and 1932, inclusive. Stanford-Binet records were available for most of those entering prior to 1924, and group test records for those who came later than 1921. All cases were tested within a few months of their entrance to the school Thus the age at which children were tested does not vary greatly. There is a slight tendency, however, for younger children of a family to be tested at an earlier age than that at which their older brothers and sisters were tested This is due to two factors: (1) Since 1928, approximately half of the children entering University High School have been admitted to the seventh and eighth grades, whereas prior to 1928 no child came into the school below the ninth grade, and (2) in some cases two or even three children from one family enter the school at the same time,

each being taken into the appropriate grade. Thus, of the siblings tested with the Stanford-Binet, the mean age at time of test was 15.7 years for those paired as older, and 15.2 for those paired as younger; of those given the group tests, the mean age at time of test was 14.4 years for those paired as older, and 14.0 years for those paired as younger. No correction has been made for any error produced by correlation between chronological age and intelligence quotient as obtained from the tests employed.

Records of University High School make it possible to verify sibship by checking father's name, mother's name, and home address of the family. Age records are obtained from the home when application for admission is made, and again from the child when tests are given. Any inconsistency is cleared up before the intelligence quotient is computed.

The cases involved in the St. Louis data were tested shortly before the completion of their eighth-grade work, and were therefore more nearly uniform in age at test than were the cases from University High School. The test employed was the Terman Group Test of Mental Ability. All eighth-grade classes of the Mason and Grant schools from May, 1926, to December, 1931, inclusive, were tested. Family groups were selected from the records by use of father's name and home address, and then submitted to the principal and two teachers, who eliminated half-sibs, as well as any other cases erroneously included.

The St. Paul cases were drawn from the records of children tested at the recommendation of public-school principals to determine whether they should be placed in special classes. The St. Paul special classes take children below IQ 80. The test used was Kuhlmann's Revision of the Binet scale. Test record forms provide name of both father and mother, together with home address. These data were supplemented by information contained in a social worker's report available for each family included.

Age at test, for cases in the Special Class Office records, instead of being approximately equal for the older and younger sibs, as was true for the two groups first described, depended only upon the age at which the principal brought the case to the attention of the Special Class Supervisor. The data employed include only cases tested between the ages of 8 and 14 years, inclusive. These limits were chosen after a study of 292 cases tested with the Kuhlmann Revi-

sion, and apparently selected at random, revealed no correlation between CA and IQ for this age range

Data from St Paul do not go back to the earliest tests given by the Special Class Office. Few cases tested before 1924 were included, as earlier records were not such as made it readily possible to verify sibship.

Selective factors determining whether or not a given pair of siblings in the population will be included in the samples are quite complex.

Families sending children to University High School average quite high in occupational status. There is probably little selection among the children of families represented, as almost none of the children from this group fail to enter high school.

The St Louis data came from two districts that are best described as average in socio-economic status The population is unusually stable in each case Small homes, owned by the occupants, predominate in the Mason district, while Grant-School pupils live mainly in two- or four-family buildings commonly owned by one of the occupants. Parochial schools draw children from both districts, but almost none go to other private schools

The St. Paul data are from a wider range of families, the majority being considerably below average in the socio-economic scale. The most important factor favoring the recording of data on a child in the Special Class Office is low school achievement, but other factors no doubt play a part It is possible that when one member of a family is sent to a special class his siblings thereby have an increased chance of being tested

It happens that almost every factor affecting the data through selection would tend to cause to be included pairs having wide differences in both age and intelligence, rather than sibs much different in intelligence, but not far apart in age For example, an important selective influence favoring the inclusion of pairs widely different, with the younger superior, is introduced by including children tested only at a given point in their progress through the school system. Thus if the older sib is retarded (and dull) in comparison with his younger brother or sister, he will be included even though the two are far apart in age, whereas when the older progresses more rapidly than the younger, one may escape being tested during the period in which the data was selected, and the pair lost. The importance of

this influence is inversely related to the length of the period over which the data are collected, and is therefore seen most in the St Louis data. Furthermore, changing conditions in the schools concerned might magnify differences for children widely separated rather than those near together in age. For example, (1) if during the years the percentage of dull children leaving school before the point where tests were given decreased; (2) if coaching became increasingly prevalent, or (3) if administration and scoring of tests were not carried on in a uniform manner throughout the period (a succession of six examiners administered the Kuhlmann test), the effect might appear as an increased difference as age interval increased. It is also probable that if any pairs that are not true siblings are included, they will be among those farther apart in age Half-sibs, both where fathers remarry (mothers' remarriage should not confuse the records unless the child takes the name of the step-father) and where paternity extends outside the family, probably average farther apart than do true siblings.

Analysis of the Data

Throughout this study the members of a family for whom data were available were paired in every possible way Thus, in families furnishing two subjects, each entered the pairing once, but when more than two subjects came from one family, each entered the pairing more than one time Since the number of entries produced by this method exceeds the actual number of cases, application of the usual formulae for probable errors or standard deviations do not give accurate values.[2] When it has been necessary to determine probable errors, the total number of entries has been employed to find a minimum value, and the actual number of cases, counting each individual once, to find a maximum value.

To gain some evidence as to how the various groups compared in resemblance, the product-moment correlation formula has been employed to calculate a coefficient of resemblance This was done in each group by entering the IQ of the older member of each pair on the x-axis, and the IQ of the younger member on the y-axis The results are presented in Table 4.

The low value of the sigmas for the St. Paul data suggests that the

[2]See Smith (19) and Burks (1)

TABLE 4

	r	Min PE	Max. PE.	σ_x	σ_y	N
U High data	49	027	029	13.15	12 00	359
St Louis data	49	.036	.041	12.75	13 75	199
St Paul data	34	026	034	10 50	10 90	465

obtained correlation be corrected for curtailment of range before it is compared with the corresponding figures for the data from the other sources [3]

The product-moment formula was next employed to discover any possible relation existing between resemblance in intelligence of siblings and the time interval separating their birth For each group of data the IQ difference for each pair has been entered on the x-axis, and the age difference in months in the y-axis.

The University High School data have been considered in two groups, the first including 278 pairs tested with group tests, and the second made up of the remaining 81 pairs tested with the Stanford Revision These data have also been combined into a single group. Since the Terman Group Test had its norms established by comparison with results obtained on the Stanford Revision,[4] this should introduce no serious error The St Louis data have been treated as a single group, and also in combination with the above. Furthermore, 11 pairs for which the age difference was greater than 78 months were eliminated (since they are affected to an extreme degree by the selective factor already pointed out), and the correlation for the remaining 188 pairs was computed. The St. Paul data, comprising a group of 465 pairs, were treated in a manner similar to that employed with the above groups The results of the above analysis, given in Table 5, furnish no suggestion that siblings far apart in age show any less resemblance than those not so widely separated

[3]The formula by Kelley (12, 13) for estimating reliability in one range when it is known in another has been applied to make this correction. The normal value for the standard deviation of Kuhlmann IQ's employed here was 16 3, and comes from an unpublished study of 428 cases that appear to constitute a random sample of children from 6 to 15, which compares fairly well with the age range of the present group Use of this figure gives a corrected r of .71. Vern James's formula gives .61 in this instance.

[4]See *Manual of Directions* accompanying Terman Group Test

TABLE 5
RELATION BETWEEN IQ DIFFERENCE AND AGE INTERVAL OF SIBLINGS
x = IQ difference
y = Age difference in months

	r_{xy}	Min PE	Max PE	M_x	M_y	σ_x	σ_y	N
U High Group	−.002	.040	.044	10.53	41.28	7.47	24.60	278
U High S-B	−.057	.075	.081	10.80	40.26	9.00	18.67	81
U High Total	*−.001*	*.036*	*.039*	*10.59*	*41.04*	*7.83*	*23.70*	*359*
St Louis	+.16	.046	.054	11.97	35.46	8.19	19.10	199
St Louis*	−.006	.049	.055	11.55	32.34	8.04	15.78	188
U High and St Louis	+.003	.029	.032	11.10	39.06	8.07	22.74	558
St Paul	+.058	.032	.041	10.86	43.20	8.10	25.56	465

*Less 11 pairs, the members of which are more than 78 months apart in age The younger members of these pairs average 14.7 points above the older in IQ

While it does not appear that the foregoing could fail to bring to light any important relationship between the factors under consideration, a second mode of analysis has been employed. Each major group has been divided into four subgroups on the basis of age interval. The mean IQ difference for each subgroup is given in Table 6 While there are slightly higher means for the wider age intervals in several cases, it will be seen from Table 7 that none of these differences meet the usual criterion for statistical significance. It is of some interest to note that the only consistent increase is in the St Louis data, where a selective factor favoring such a relationship has been most potent

While the data available do not include birth order of the subjects, there is in Table 3 material that permits a comparison of the average intelligence of the older and younger members of the family groups included The possible comparisons are given in Table 8.

The difference found in the University High data is too small to be given any weight alone, nevertheless, it is in the same direction as both the others. In the St. Louis data, the manner in which test records were obtained, as has been previously indicated, strongly favors including pairs in which there exists a superiority of the younger. The same factor has been of influence in the University High School data, but not to the same extent, as the testing covered a much longer period. The St Paul data are probably immune from this influence. Here the factors that might play a part are far

TABLE 6

MEAN IQ DIFFERENCE OF SIBLING PAIRS SEPARATED BY VARIOUS AGE INTERVALS

Age interval	U High Mean	N*	St Louis Mean	N	U High and St Louis Mean	N	St Paul Mean	N
12-23	10 63 ± 54*	93	10 97 ± 62	64	10 77 ± 32	157	9.55 ± 45	126
24-35	10 67 ± 51	103	11 43 ± 61	64	10 96 ± 40	167	10 79 ± 52	103
36-71	10 64 ± 58	124	12 51 ± 69	58	11.24 ± 43	182	11 87 ± 42	173
72-up	10 11 ± 72	39	17 19 ± 2 27	13	11 88 ± 80	52	10 83 ± 70	63

*The probable errors given in this table are based on the values of N which accompany them N is the number of pairs, and is not the same as the number of individuals Since the actual number of older or of younger siblings is less than the number of pairs, the reported P.E values are minimum values

TABLE 7

A COMPARISON OF THE MEAN IQ DIFFERENCES OF GROUPS OF SIBLINGS SEPARATED BY VARIOUS AGE INTERVALS

M_1—Mean IQ difference of pairs 12-23 months apart
M_2— " " " " " 24-35 " "
M_3— " " " " " 36-71 " "
M_4— " " " " " 72 or more months apart

	D	P E min.	D/P.E min
U High data			
$M_2—M_1$	04	74	05
$M_3—M_1$	01	79	01
$M_4—M_1$	—52	90	57
$M_3—M_2$	—03	77	04
$M_4—M_2$	—56	88	64
$M_4—M_3$	—53	92	58
St Louis data			
$M_2—M_1$	46	87	.52
$M_3—M_1$	1 54	93	1 66
$M_4—M_1$	6.22	2.35	2 65
$M_3—M_2$	1 08	92	1 17
$M_4—M_2$	5 67	2 35	2.41
$M_4—M_3$	4 68	2 37	1 97
U High and St Louis data combined			
$M_2—M_1$	19	51	38
$M_3—M_1$	.47	.54	.87
$M_4—M_1$	1 46	86	1.70
$M_3—M_2$	28	59	47
$M_4—M_2$	1 27	.89	1.43
$M_4—M_3$	99	.50	1 98
St Paul data			
$M_2—M_1$	1 24	69	1.80
$M_3—M_1$	2.32	62	3 74
$M_4—M_1$	1 28	83	1 54
$M_3—M_2$	1 08	67	1 61
$M_4—M_2$	04	87	.05
$M_4—M_3$	—1 04	82	1 27

TABLE 8

DIFFERENCES IN INTELLIGENCE OF OLDER AND YOUNGER SIBLINGS

M_1=Mean IQ of older
M_2=Mean IQ of younger

		D	P E Min	P E Max	D/P.E Min.	D/P E Max	N
U High data	$M_2—M_1$	0 40	64	72	63	56	359
St. Louis data	$M_2—M_1$	5 25	90	1 00	5 83	5 25	199
St Paul data	$M_2—M_1$	5 15	47	59	10 96	8 73	465

more complex, so that there is no certain means of determining whether or not the observed difference in intelligence favoring the younger is due to the manner in which the data are selected

Conclusions

The major conclusion from the results of the foregoing study, based on mental test data for more than one thousand pairs of siblings, separated by age intervals ranging up to eleven years, is that any relation between age interval and resemblance in intelligence is not of sufficient magnitude to be revealed by the methods employed.

Among the two groups of siblings that are probably representative in degree of resemblance of sibs who have reached junior high school, the resemblance is expressed by a correlation of .49

It was also observed that in certain of the data there was a slight superiority of younger over older siblings, within the family groups that were available. It is of interest to note that, in the data most nearly free from known selective factors favoring the inclusion of pairs in which the younger is superior, no significant difference between younger and older siblings exists.

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College of Education and
Institute of Child Welfare
University of Minnesota
Minneapolis, Minnesota

UNE ÉTUDE DE LA RELATION ENTRE L'INTERVALLE SÉPARANT LES ÂGES ET LE DEGRÉ DE RESSEMBLANCE INTELLECTUELLE DES ENFANTS AYANT LES MÊMES PARENTS

(Résumé)

Le problème de l'enquête, c'est de déterminer, dans les limites dans lesquelles l'intelligence est mesurée par les tests employés, si le degré de ressemblance intellectuelle entre des pairs d'enfants nés aux mêmes parents et l'intervalle qui sépare la naissance des enfants ont quelque relation l'un à l'autre. Les données se composent des résultats de tests mentaux pour 1023 paires d'enfants de race blanche, nes aux mêmes parents et dans ce pays, faisant partie de 614 familles, comprenant 1401 individus. On a choisi les données d'entre trois états socio-économiques, comprenant un groupe d'intelligence inférieure, un deuxième d'intelligence moyenne, et un troisième d'intelligence supérieure. Les deux membres de chaque paire ont subi le même test dans des conditions semblables dans tous les cas.

Pour chaque groupe de données la correlation entre l'intervalle séparant les âges et la différence du QI s'est montrée approximativement nulle. La différence moyenne du QI pour les paires beaucoup séparées au point de vue de l'âge n'est pas plus grande d'une façon signifiante que celle pour les paires non ainsi separées. Le degré de ressemblance entre les deux groupes où les influences de sélection ont eté de la moindre importance s'exprime par une corrélation de 0,49, ce qui ressemble de près aux résultats obtenus dans les enquêtes antérieures. On a remarqué que les jeunes enfants nés aux mêmes parents se sont montrés un peu supérieurs aux plus âgés dans certaines données. Il n'existe aucune différence semblable dans les données qui ont subi le moins des influences connues de sélection.

FINCH

EINE UNTERSUCHUNG DER BEZIEHUNG DES ALTERSABSTANDES ZUM GRAD DER ÄHNLICHKEIT ZWISCHEN GESCHWISTERN, IN BEZUG AUF INTELLIGENZ

(Referat)

Es war die Aufgabe der Untersuchung, zu bestimmen, ob, innerhalb der Grenzen, zwischen denen sich die Intelligenz durch die hierzu verwendeten Tests messen lasst, eine Beziehung besteht zwischen dem Grad der Ahnlichkeit, in Bezug auf Intelligenz, zwischen Paaren von Kinder der selben Eltern einerseits und dem Zeitabstand zwischen den Geburten der Kinder anderseits. Die Daten bestehen aus Intelligenzprufungsprotokollen von 1023 eingeborenen (Amerikanischen) weissen Geschwisterpaaren aus 614 Familien, 1401 Individuen einschliessend. Die Daten stammen aus drei verschiedenen sozial-okonomischen Schichten und schliessen eine niedrigere, eine mittelmassige, und eine hochstehende Gruppe, in Bezug auf Intelligenz, in sich ein. Die beiden Mitglieder jedes Paares wurden in jedem Fall mit dem selben Test und unter ahnlichen Bedingungen gepruft.

In jeder Protokollengruppe erwies sich die Korrelation zwischen den Altersunterschieden und den Intelligenzunterschieden als ungefahr Zero. Der durchschnittliche Unterschied zwischen den Intelligenzquotienten ist bei Paaren mit grossem Altersunterschied nicht bedeutend grosser als bei Paaren, daren Mitglieder einander in Bezug auf Alter nahe stehen. Der Grad der Ähnlichkeit bei zwei Gruppen in denen auslesende Einwirkungen am wenigsten wichtig gewesen sind, lasst sich durch die Korrelationzahl .49 ausdrucken. Dieser Befund geht mit den Befunden aus fruheren Untersuchungen eng parallel. In einigen Befunden zeigte sich eine geringe Überlegenheit der jungeren uber die alteren Geschwister. Bei den Daten die am freisten von erkannten selektiven Einwirkungen waren zeigte sich kein Unterschied dieser Art.

FINCH

RESPONSES OF BOYS BETWEEN THE AGES OF FIVE AND SIXTEEN YEARS TO HULL'S POSTURAL SUGGESTION TEST*[1]

From the Department of Child Welfare, White Plains, New York

RAMONA MESSERSCHMIDT

SUBJECTS AND PROCEDURE

Hull's postural suggestion test (1) was run with 194 school boys from age 5 through 16 years. The subjects were within 6 months of the birthday for the following ages: 5, 6, 8, 10, 12, 14, and 16. There were from 25 to 32 boys for each of the 7 age groups taken from the two Greenburgh, the two Elmsford, and the Dobb's Ferry Public Schools in New York State. Experimentation was carried on at three different sessions: in February of 1931, September of 1931, and January of 1932. No subject was aware of the true nature of the test, and all were excluded who showed any timidity or apprehension in regard to the situation.

The essentials of the method and apparatus employed have been described in some detail elsewhere (1). Although the present investigation adhered as rigidly as possible to the procedure used with adults, some modifications were necessary. By means of a vocabulary test of 30 words given to 75 5-year-olds, words of the original directions that were not understood by children of this age group were detected and appropriate synonyms substituted. By preliminary trials with 10 children at both the 5- and 10-year levels, it was noted that blindfolding provoked a mild fear response whereas the same subjects were willing enough to close their eyes and religiously keep them shut. It was also discovered that some sort of introductory explanation was necessary partially to dissipate vague fears and to satisfy curiosity regarding the nature of the experiment

At each school a vacant room was used. The apparatus was

*Recommended by Clark L Hull and accepted for publication by Carl Murchison of the Editorial Board

[1]The writer wishes to express her gratitude to Professor Clark L Hull for generous assistance in the design, formulation, and interpretation of this study. Professor Gardner Murphy recommended a vocabulary test for the youngest children.

placed on a table and hidden from the boys by a cardboard screen. After each subject had entered and approached an appropriate spot before the screen, he was given the following preliminary explanation "I am going to have you stand with your eyes closed for a few minutes And I will tell you why Some boys go frontward a little when they stand with their eyes closed, some go backward a little, and some stand very still. Now I want to see how you stand when your eyes are closed You won't mind, will you?" Those boys who did not willingly agree at this point were excused Agreement obtained, the examiner continued "Now close your eyes and don't open them till I tell you." As each boy stood quietly erect, his eyes tightly shut, the experimenter stood before him and repeated in a quiet, confident tone: "You are falling frontward. You can't help yourself You are falling frontward. Farther frontward. Frontward. Frontward. Farther yet. You are falling frontward still more. More yet. More. More. More yet. Frontward, etc." A pause of two or three seconds was made at each period. Close watch was kept of the subject's spontaneous tremors and, at each major forward movement, the experimenter said, "There you come." She then went on with the suggestion as described above.

Without his knowledge, a pin with a tiny hook at its end was lightly caught into the fabric of the garment over the subject's right shoulder To this pin was attached a thread which ran back about 4 ft and over a simple, easy-running system of flanged aluminum pulleys. From this pulley system there was suspended by means of a second thread a small steel rod which moved in a vertical sleeve and to the lower end of which was attached a stylus The horizontal postural movements of the subject were traced by the stylus on the smoked paper of a kymograph as a vertically oscillating line, the oscillations in the tracing being reduced to one-third of their actual amplitude. One of these tracings, considerably reduced, is reproduced as Figure 1.

The experimental procedure involved running each S for 14 minutes. In accord with the preliminary explanation, there was a 2-min interval, after starting the drum of the kymograph, during which the S stood quietly. Verbal suggestion was then used for 4 min., followed by a 2-min. quiet period, verbal suggestion for 4 min, and a final 2-min quiet period.

The degree of suggestibility of any S may be measured by two

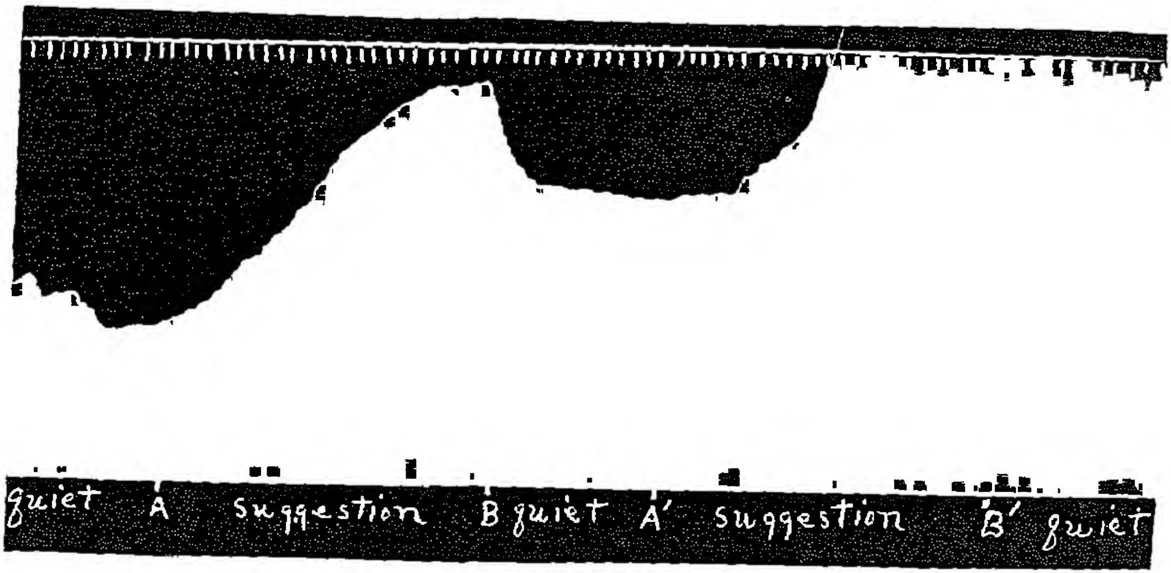

FIGURE 1

TYPICAL RECORD FROM THE EXPERIMENT TO DETERMINE THE SUGGESTIBILITY OF A SUBJECT

The upper line gives the time in 5-second intervals The middle line shows the forward postural movements of the S in response to suggestion The lower line is for signals, showing the point at which the suggestion begins, *A* and *A'*, and the approximate point at which it is terminated, *B* and *B'* Note that during the second suggestion period the middle line goes off the record This is typical representation of a maximum response

criteria, the magnitude of the response and the speed of the response, the S being regarded the more suggestible, the greater the amplitude of the response and the shorter the time required to evoke it.

Precise quantitative tendencies to sway forward or backward in response to the different forms of suggestion are rather obscured by the swaying tremors which are continuous and quite large in all persons, especially when standing with closed eyes. This produces in the recording of the postural movements an irregular up-and-down tracing which makes it difficult to measure the exact distance swayed forward or backward under the influence of suggestion at any given time. A middle line was, therefore, estimated through the tracings recorded during the first 2 minutes to act as a convenient base line from which the forward and backward swaying movements could be measured. To insure accuracy, each record was tacked to a straight board of soft wood and a T-square used in drawing. The different periods were brought into relief by drawing straight lines to separate them.

The method by which the magnitude and speed of the suggestion response were secured may best be explained by referring to Figure 1. The upper line is the time record, a notch marking the lapse of every 5-second period. The second line shows the forward-backward postural movements, the sharp peaks representing the maximum suggestion response. The lowest line gives, by means of notches, the exact points at which the suggestive stimulation begins and ends. After the record had been shellacked, a perpendicular white line was drawn to set off the different periods. The exact point at which the first suggestion stimulation begins is marked by *A* and where it ends by *B*, the second suggestion stimulation begins at *A'* and ends at *B'*. Points representing the maximum suggestion responses are indicated by the number *1* for the first suggestion period and by the number *2* for the second suggestion period. The amplitudes of the suggestion responses were measured on the tracings in units of 1/50 inch. Since a tall person, for a given amount of angular sway, will move farther at the level of his shoulders than a short person, a correction needs to be made. The formula as given by Hull is

$$S.A.I. = \frac{100A}{H}$$

where $S.A.I.$ = suggestion amplitude index,
A = recorded amplitude of postural movement in units of 1/50 inch,
H = standing height of subject in inches.

The suggestion time is the distance between the point where the suggestion stimulation begins, namely *A* and *A'*, and the point of maximum sway as represented by *1* and *2*.

Many children responded so grossly that their reaction no longer recorded on the kymograph. These were accordingly termed maximum suggestion responses and given a numerical S.A.I. value of 360 for the reason that the largest recorded S.A I came to 356.43. Those showing a negative reaction were given a minus score in contrast to the plus scores for showing positive suggestibility. When there was no perceptible change in the postural sway during the period of suggestion stimulation, the S A I. was scored as zero. This type of responsiveness was rare but did occur occasionally

Results

Table 1 shows the relationship of age to suggestibility as measured both by the magnitude and the speed of response Figure 2 also

TABLE 1

Average Responsiveness of Boys to Hull's Postural Suggestion Test as a Function of Age

Magnitude of responsiveness

Age in years	S A I. for first suggestion stimulation	S.A I. for second suggestion stimulation	S.A I for the two periods
5	56 30	59 34	57 82
6	200 92	217.97	209 44
8	220.87	236.37	228.62
10	151 13	133.44	142 28
12	134 67	133.58	134 12
14	100 04	30 77	90 40
16	62 99	98 12	80 55
Mean	132.42	137.08	

Speed of responsiveness

Age in years	Suggestion time for first suggestion stimulation	Suggestion time for second suggestion stimulation	Suggestion time for the two periods
5	60 40	56 72	58 56
6	56.38	41.77	49.07
8	55 95	40 62	48 28
10	53.86	44.66	49 26
12	50 96	51 28	51 12
14	72 10	54 32	63 21
16	63 80	53 44	58.62
Mean	59 06	48 97	

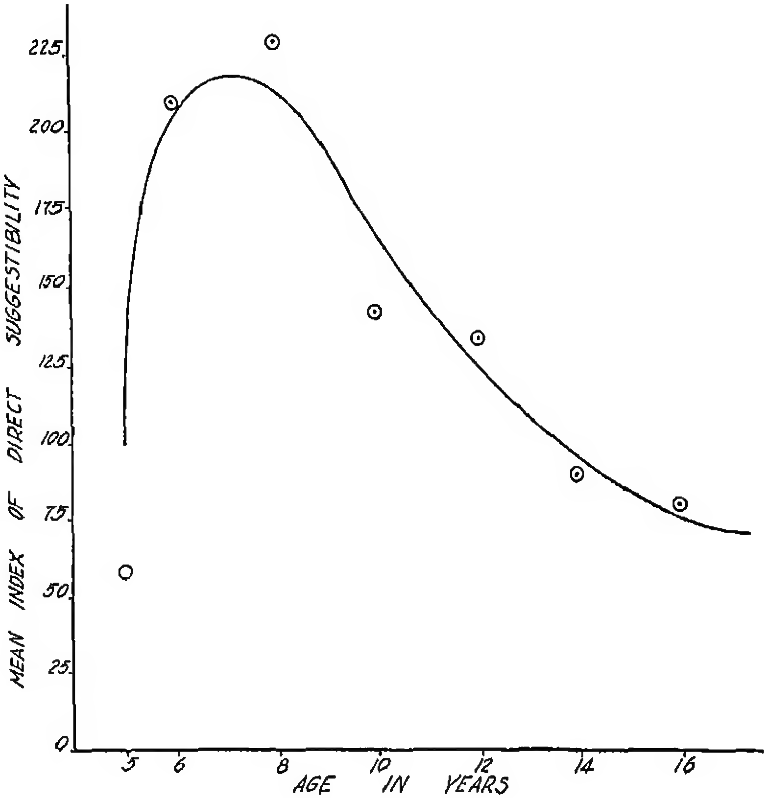

FIGURE 2
Graph Showing Responsiveness of Boys to Hull's Postural Suggestion Test as a Function of Age

represents the magnitude of responsiveness as a function of age The mean S A I 's in Table 1 and Figure 2 show suggestibility increasing from 5 through 8 years with a gradual decrease from 8 years on. Continuous decrease in suggestion response below 8 years indicates that suggestibility will disappear entirely with very young children, no habit patterns having as yet been formed. Phillipe and Clavière (4) noted a similar lessening of response to suggestion in children from 3 to 7 and concluded that it disappeared entirely with very young children Thus the present investigation and a companion

study reported elsewhere (2) yield substantially the same results in regard to suggestibility as a function of age, and these findings are corroborated by those of previous investigators.

Table 2 indicates the same general trend for suggestibility to decrease gradually after the second grade, although the high S.A.I for the sixth-graders blurs the clarity of this tendency. The wide age range per grade is a complicating factor which undoubtedly accounts for the lack of a clear-cut tendency for suggestibility to decrease with grade increase The present study does not show such a definite tendency for suggestibility decrease in the higher grades as do some other investigations.

What is the relationship of the suggestion responses for the two

TABLE 2
AVERAGE RESPONSIVENESS OF BOYS TO HULL'S POSTURAL SUGGESTION TEST AS A FUNCTION OF GRADE

Magnitude of responsiveness

Grade	No. per grade	S A I for first suggestion stimulation	S A I for second suggestion stimulation	S A I for the two periods
Kdg	23	93 88	95.35	94 61
1	27	214 54	223 38	218 96
2	19	184 74	174 59	179 66
3	15	193 76	200 27	197 01
4	19	180 43	195 09	187 16
5	19	136 02	101 00	118 51
6	14	199 28	226 52	212 90
7	10	70 10	41 94	56 02
8	19	77 85	88 31	83 08
9	10	128 71	204 66	102 33
10	15	52 98	186 11	115 80

Speed of responsiveness

Grade	No. per grade	Suggestion time for first suggestion stimulation	Suggestion time for second suggestion stimulation	Suggestion time for the two periods
Kdg	23	58 56	107 39	82 97
1	27	104 88	141 95	70 97
2	19	65 94	121 94	60.97
3	15	51 07	88 00	44 00
4	19	52 00	90 73	45 36
5	19	67 36	118 37	59.18
6	14	51 00	122 21	61 10
7	10	51 20	110 10	52 05
8	19	53 10	95 25	47 62
9	10	52 00	110 79	55 85
10	15	80 33	139 46	69 73

periods? Comparing the amplitudes of the two periods for the group as a whole, there is little difference between the amplitudes of the first and second periods. The S.A.I.'s for the first suggestion stimulation are 97.1% as great as those for the second suggestion stimulation, the difference being so slight as to be quite negligible. Examining the ages separately, there is little appreciable difference at any age with the exception of the 16-year-olds, the mean S.A.I. for the first period being only 64.2% of the mean S.A.I. for the second. The 5- through the 8-year-olds are slightly more suggestible during the second period, the 10- through the 14-year-olds are slightly more suggestible in the first period. Correlations were computed for each of the three ages, 5, 8, and 16. All show a high positive relationship between S.A.I. (I) and S.A.I. (II). The 8-year-olds, or the most suggestible group, show the greatest degree of consistency between the two suggestibility indices. For this age, a positive correlation coefficient of .92 was obtained with a probable error of .019. The relationship was not quite so high for the 5- and 16-year-olds, although each yielded a very satisfactory positive *r*. There was a positive *r* of .84 with a P.E. of .085 for the 5-year-olds and a positive *r* of .83 with a P.E. of .044 for the 16-year-olds.

There is a little more variation in the suggestion time between the two periods than in the S.A.I.'s. The mean suggestion time for the group is definitely less for the second period, it being 82.9% as great as the time taken to evoke the maximum response during the first period. Each age, with a slight variance at 12 years, yielded more rapidly to suggestion during the second stimulation. It is interesting to note that the most suggestible ages (6 and 8 years) show the greatest difference between the two periods, the suggestion times for the second periods being 74% and 72.2% of the first, respectively, for the 6- and 8-year-olds. These results agree with those reported by Patten, Switzer, and Hull for adult subjects (3).

As might be expected, from Table 3, we see the 6- and 8-year-olds giving the highest percentage of maximum responses. At these two years over 50% of the children showed a maximum responsiveness, while with the youngest, the least suggestible, only 16% of the group yielded maximally. From 8 through 16 there was a gradual lessening in maximum responsiveness. Not only a quantitative but also a qualitative difference was discernible in the typical maximum responsiveness at different ages. A few of the children at 5 and 8 years began twisting about or swaying from side to side until the

TABLE 3
RELATIONSHIP OF MAXIMUM RESPONSES TO AGE

Age	Percentage per age showing maximum responses	Percentage per age showing each type of maximum responses: Constant motion—either twisting about or swaying *from* side to side	Touching the *floor*—subject either lying flat on the floor or bending so low that his head touches the floor	Walking—either to the wall opposite and remaining or constant circling of the room	Stooping—approximating a *jack-knife* position	Leaning over until balance is lost, repeating many times Leaning over then taking a step or two forward, repeating many times	Leaning considerably *but not* enough for loss of balance
5	16	4		12			
6	56		25	25	6		
8	59	5	20	20	14		
10	37			21	10	3	3
12	40			12	16	12	
14	28					17	11
16	28			3		11	11

stylus was jerked off the kymograph drum. Once started, they kept up a constant motion throughout the entire suggestion stimulation. Twenty-five per cent of the 6-year-olds and 20% of the 8-year-olds *either lay flat upon the floor or bent so low that the head touched* the floor The types of behavior so far described ceased after 8 years and were, therefore, regarded as the lowest order of maximum responses The highest percentage of maximum responses from 5 through 10, several at 12, and a few at the 16th year were locomotor in nature. Some subjects walked to the opposite wall where they *halted and remained until* the suggestion stimulation ceased, while others kept constantly circling the room The majority of those giving maximum responses at 12, a fair number at 10, and 8, and a few of the 6-year-olds, stooped until they took a positon which approximated a jack-knife. Most of those at 14, many at 16, quite a few at 12, and a few at 10, either leaned over until they lost their balance, *repeating this procedure many times*, or leaned over until it

TABLE 4
NEGATIVE RESPONSIVENESS OF BOYS TO HULL'S POSTURAL SUGGESTION TEST AS A FUNCTION OF AGE

Age	Percentage for 1st suggestion period	Percentage for 2nd suggestion period	Mean percentage for the two periods	Negative S.A. I.'s for 1st period	Negative S.A I's for 2nd period	Mean negative S A I's for the two periods
5	32	36	34	—122	—173	— 48
6	9 67	16 12	12 89	— 62	— 88	— 75
8	6.82	6 82	6 82	— 46	— 50	— 48
10	26 66	36 66	31 66	—141	—326	—234
12	36	48	42	—237	—335	—286
14	39 28	50	44 64	—274	—326	—300
16	46 15	65.34	55 74	—318	—500	—409
Mean	28 08	36 85	32 53	—171 43	—256 85	—242 14

became necessary to take a step or two forward to keep from falling, this being kept up until suggestion ceased. As many of the 16-year-olds as showed the last type of maximum response, almost as many of the 14-year-olds, and a few at the 10-year level leaned over considerably but not enough for loss of balance.

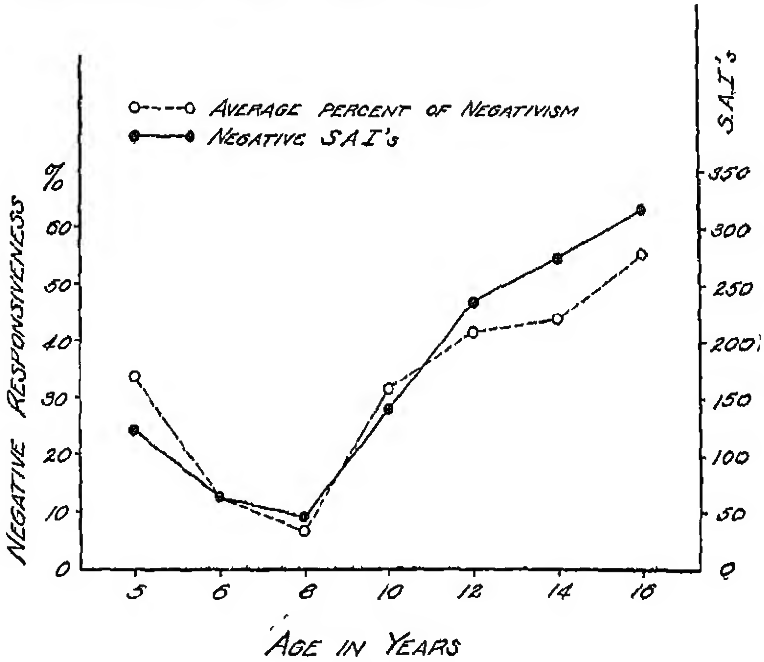

FIGURE 3
GRAPH SHOWING NEGATIVE RESPONSIVENESS OF BOYS TO HULL'S POSTURAL SUGGESTION TEST AS A FUNCTION OF AGE

Table 4 and Figure 3 give the percentage and degree of negativism as related to age. As might be expected, those age groups showing the greatest degree of positive suggestibility show the least negative responsiveness. One-third of the 5-year-olds were negatively affected, with a definite decrease at both 6 and 8 years, the 8-year-olds showing only a slight negativism. From 8 years on there was a rapid regularity of increase in negativism until a little over one-half of the 16-year-olds gave a negative response. Percentage and degree of

negativism, therefore, show a very definite and logical trend, the curve of negativism in Figure 3 being the distinct opposite of the curve of suggestibility in Figure 2. Those negatively suggestible displayed a definite tendency to show a preliminary mild response followed by a second reaction of considerably greater magnitude The mean percentage of negativism for the first period was only 75 9% of the second, while the mean S A.I 's for the first suggestion were only 66.7% of those for the second suggestion.

TABLE 5
SUM OF POSITIVE SUGGESTION RESPONSES AS A FUNCTION OF AGE

Age	S A I (I)	S A I. (II)
5	1341	1628
6	5676	6699
8	5599	6538
10	4469	4320
12	3766	3764
14	3110	3122
16	3223	2788
Mean	3896	4837

Table 5 gives the sum of the positive S A I.'s by age groups for the two suggestion periods The mean S A.I. value of the first period is 80 5% of the second. Comparing this to the ratio of 66 7% for those negatively suggestible, and remembering the high positive correlation for the 8-year-olds (the most suggestible group), it may be seen that subjects showing a marked positive suggestibility during the first period are more likely to respond in similar degree to the second suggestion than are those giving mild or negative response the first period

Only three of the subjects failed to respond negatively or positively during one of the two suggestion periods, while no one re-

TABLE 6
PERCENTAGE OF OPPOSITE RESPONSES PER AGE

Age	Percentage of opposites
5	16
6	0
8	0
10	13
12	25
14	21
16	16

TABLE 7

Age group	Types of initial responses for first period: Latency, Av %	Latency, Av time seconds	Shallow negative, %	Shallow negative, Time seconds	Shallow positive, %	Shallow positive, Time seconds	Types of after-effects for first period: After discharge, %	After discharge, Time seconds	Compensatory after-response, %	Compensatory after-response, Time seconds	Slight carry-over, %	Slight carry-over, Time seconds	Recovery, %	Recovery, Time seconds
5	48	35.3	12	21	4	7	40	47.7	40	39.8	44	36.2	24	50.1
6	25.8	18	38.7	60	3.2	67	77	57.6	6.1	41.2	19.3	55.1	6.5	60
8	41	14	27.5	11	3.2	5	75.8	55.7	3.4	25	17.2	42	13.7	43.3
10	33.3	17.3	36.6	17	13.3	33.3	76.6	55.8	23.3	50.1	10	36.3	3.3	60
12	48	10	28	16	12	26.3	76	49.9	16	48.3	16	44.2	16	44
14	37.2	38	17.8	30	28.5	17	71.4	43	35.7	36.3	17.8	28.6	18.5	43
16	61.5	27.7	42.3	24	3.8	6	65.3	53	23.0	44.5	30.7	36.1	7.7	31

mained completely unaffected One of the 5-year-olds who at first showed a slight negative response obtained a zero score during the second suggestion stimulation, while another S at this same year gave no reaction except a slight backward sway during the second period. Another at 14 gave a mild positive response in the second period after obtaining an initial score of zero.

Opposite responsiveness also was rare as seen in Table 6. At the most suggestible ages, 6 and 8, none of the subjects shifted from a positive to a negative reaction. With practically all subjects influenced oppositely, there was at first a mild positive response followed by a mild negative one.

Hull has pointed out various typical behavior patterns to waking suggestion in his preliminary observations of adults. He found a large percentage of subjects showing an initial *shallow negative* reaction which gave place to a marked positive reaction several times larger when the suggestion stimulation was continued. Others showed a period of about the same duration following the beginning of the suggestion but preceding the clear positive reaction during which there was no noticeable response. This tendency to inertia was termed *latency.* Corresponding to this latency, he found, following the termination of suggestion, a similar period during which the response was undiminished or actually increased Hull indicated that this latter phenomenon corresponds to the *after-discharge* found in spinal reflexes of dogs (1).

The subjects in the present investigation showed three general types of initial responses, namely, a first shallow negative followed by a marked positive, a first shallow positive followed by a marked negative, and a latency period. The after-effects were typified as after-discharge, complete recovery from suggestion, a slight carry-

TABLE 8
CORRELATIONS

	16 years		*8 years*		*5 years*	
	r	P.E.	*r*	P E	*r*	P E.
Amplitude to latency	19	13	14	12	.24	13
Amplitude to after-discharge	05	00099	63	.07554	43	.11
Amplitude to recovery	19	13	33	109	.03	13
Amplitude to slight carry-over	37	.11	21	12	.09	.13
Amplitude to compensatory after-response	.27	.12	21	12	19	.13
Latency to after-discharge	13	.13	.04	.125	20	005

over of the suggestion, and a compensatory response opposite to that shown during the suggestion stimulation The average percentage and average time per age for these initial and after-responses to suggestion are given in Table 8 Only responses for the first suggestion period are represented. There was a wide variation between the two suggestion periods for latency During the second period only 13 2% as many children showed latency as exhibited it during the first Only 60% as many gave a compensatory after-response in *the second period as in the first, and for the second period only 17% of the first gave an initial shallow negative before a positive.* It seems reasonable to expect the least degree of latency for more suggestible age groups. An examination of the data in columns one and two of Table 8 suggests that this does hold true, but only slightly. The r's and P E.'s indicate little or no relationship between amplitude and latency There is also little relationship between latency and after-discharge For the 8-year-olds a fairly satisfactory positive r of .63 was obtained between amplitude and after-discharge, with a mild relationship for the 5-year-olds with a positive r of 43 Little relationship is indicated between amplitude and recovery, amplitude and slight carry-over, or amplitude and compensatory after-response *As judged by these r's in Table 8, the 5-year-olds show more recovery from influence of suggestion than any other age while more of the 8-year-olds* recovered than those at the 16-year level Both the 5- and 16-year-olds tended to show more of a slight carry-over than any of the other ages, the same being true for compensatory after-response. A large percentage of the 14-year-olds also showed the latter.

Dividing the subjects into significant age groupings in accord with their scores, and using the U-curve as a measure of prestige, it was found that the prestige factor was operative for the group as a whole The influence of this factor was most marked for the 10- and 12-year-olds and was present in mild form with 14- and 16-year-olds.

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Department of Child Welfare
White Plains, New York

LES RÉPONSES DES GARÇONS ENTRE LES ÂGES DE CINQ ET DE SEIZE ANS AU TEST DE SUGGESTION DE POSTURE DE HULL

(Résumé)

On a fait subir le test de suggestion de posture de Hull à 194 écoliers Les sujets n'ont pas été éloigné plus de six mois de l'anniversaire pour les âges suivants 5, 6, 8, 10, 12, 14 et 16 Le procédé expérimental s'est composé de mettre chaque sujet à l'épreuve pendant quatorze minutes Il y a eu deux périodes de suggestion de quatre minutes chacune pendant lesquelles on a donné à chaque sujet des directions verbales directes de tomber en avant, toutes deux précédées et suivies d'une période tranquille d'une durée de deux minutes On a mesuré le degré de suggestibilité et par la magnitude et par la vitesse de la réponse. Les deux mesures de suggestion montrent une suggestibilité qui s'accroît de cinq à huit ans avec une décroissance graduelle à partire de huit ans. Une décroissance continue de la réponse à la suggestion aux âges moins de huit ans indique que la suggestibilité disparaîtra entièrement chez les très jeunes enfants, puisqu'ils n'ont formé aucunes formes d'habitudes Il y a plus de variation du temps de suggestion entre les deux périodes de suggestion que dans la magnitude de la réponse Le temps moyen de suggestion et définiment plus petit pour la deuxième période, étant moins grand de 82 9% Il est intéressant de noter que les âges où la suggestion a le plus de force (six et huit ans) montrent la plus grande différence entre les deux périodes, les temps de suggestion des secondes periodes étant moins grands que ceux des premières de 74% et de 72.2%, respectivement, pour les garçons âgés de six et de huit ans. Les réponses négatives dans leur rapport avec l'âge montrent une tendance très définie et logique, la courbe de négativisme étant le contraire distinct de la courbe de la suggestibilité positive

Messerschmidt

REAKTIONEN VON KNABEN IM ALTER VON FÜNF BIS SECHSZEHN JAHREN AUF HULLS TEST DER HALTUNGSSUGGESTIONEN (HULL'S POSTURAL SUGGESTION TEST)

(Referat)

Man gab 194 Schuljungen Hulls Test der Haltungssuggestionen. Die Versuchspersonen befanden sich innerhalb einer sechsmonatlichen Spanne des Geburtstags für die folgenden Altersjahre 5, 6, 8, 10, 12, 14 und 16. Das experimentelle Verfahren bestand in der Prüfung jeder Vp für vierzehn Minuten. Es gab zwei Suggestionsperioden von je vier Minuten, während derer die Vp direkte mündliche Aufträge erhielten, nach vorwärts zu fallen, wobei diesen Perioden je eine stille Periode von zwei Minuten vorausging und folgte. Man bestimmte den Grad der Empfänglichkeit sowohl durch das Ausmass wie durch die Geschwindigkeit der Reaktionen. Beide Masse der Suggestion zeigen, das die Empfänglichkeit vom fünften bis zum vollendeten achten Altersjahr wächst, und vom achten aufwärts allmählich abnimmt. Die kontinuierliche Abnahme der Reaktion auf Suggestionen unter dem achten Altersjahr zeigt, dass die Empfänglichkeit für Suggestionen bei sehr jungen Kindern ganz verschwindet, wo noch keine Gewohnungsvorbilder geformt worden sind. Es lassen sich grössere Variationen in der Suggestionszeit zwischen den zwei Suggestionsperioden als im Ausmass der Reaktion feststellen. Die mittlere (mean) Suggestionszeit ist bestimmt geringer für die zweite Periode, sie beträgt 82,9% derjenigen der ersten Periode. Es ist interessant festzustellen, dass die empfänglichsten Alter (sechs bis acht Jahre) die grösste Differenz zwischen den zwei Perioden aufweisen, die Suggestionszeiten der Sechs- und Achtjährigen für die zweite Periode betragen 74 Beziehungsweise 72,2% der der ersten. Negative Reaktionen auf das Alter bezogen zeigen eine bestimmte und logische Tendenz, die Kurve des negativen Verhaltens sind das deutliche Gegenteil der Kurve der positiven Empfänglichkeit.

MESSERSCHMIDT

THE SUGGESTIBILITY OF BOYS AND GIRLS BETWEEN THE AGES OF SIX AND SIXTEEN YEARS*[1]

From the Department of Child Welfare, White Plains, New York

RAMONA MESSERSCHMIDT

INTRODUCTION

There has been considerable investigation to gauge the suggestibility of children With a few exceptions, the results of these investigations have been quite uniform. Using a variety of tests, Bell (2), Binet (3), Brown (4), Chojecki (5), Scott (16), Seashore (17), and Town (21) found that individuals differ in degree of suggestibility for different situations rather than in being generally suggestible or non-suggestible. Aveling and Hargreaves (1), on the other hand, concluded that suggestibility is a general characteristic because their subjects responded more or less typically to various tests They, too, noted that with some individuals the degree of susceptibility to suggestion varied with different situations

All those who have studied the suggestibility of children have found it to vary with age Aveling and Hargreaves (1), Binet (3), Cohn and Dieftenbacher (6), Otis (15), and Small (19) report that suggestibility decreases regularly with increase in age Gilbert (9), Giroud (10), and Guidi (11) report that suggestibility increases from 6 to 9 years, then lessens progressively after the 9th year.

Grade comparisons have yielded results similar to those of age groups, suggestibility decreasing regularly with grade increase as pointed out by Binet (3), Otis (15), and Small (19)

Jones (13), using bright and retarded children of the same mental age, found suggestibility to vary with chronological age more than with mental age, from which she concludes that experience and training are important factors. Small (19) observed, as well, that

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suggestibility lessens as subjects gain in experience and training. The study of Cohn and Dieffenbacher (6) brings out this same tendency. Binet, also, implied as much.

Most investigators agree that there is little correlation between suggestibility and intelligence.

The following report women and girls to be more suggestible than men. Brown (4), Dressler (7), Gilbert (9), Seashore (17), Starch (20), Wolfe (23), and Yung (24) Only one author, Otis (15), reports girls slightly more suggestible than boys, while Cohn and Dieffenbacher (6) believe that there is no sex difference The Otis test is questioned on the basis of the high positive correlation obtained with scores on intelligence tests which is quite atypical.

Importance of the prestige of the experimenter in suggestion studies has been recognized by many investigators Binet and Henri (3, p. 62), Binet and Vaschide (3, p. 24), Giroud (10), and Vitali (22) called attention to the prestige factor, but there was little clear experimental distinction with and without prestige until the studies of Aveling and Hargreaves (1), which were later followed through by Estabrooks (8) In these last two studies, where personal or prestige suggestion was used, the subjects tend to fall into two sharply divided groups, namely, the suggestible and the non- or contrasuggestible. This results in a U-curve as contrasted with the ordinary symmetrical curves obtained when the prestige factor is not present Where suggestion is impersonal the distribution is approximately normal, most subjects being moderately suggestible.

The present study comprises 11 tests which are considered to be representative of previous investigations with suggestion. A second study (14) employs Hull's postural suggestion technique (12). Points of likeness and variance between the results of these two studies are to be noted and present conclusions compared with those of previous investigators

Subjects and Test Battery

This battery of suggestion tests was given in the spring of 1927 to a group of unselected school children taken from Eighth Avenue School, Indianola Junior High School, and a few from East and North High Schools in Columbus, Ohio The age range was from 6 through 16 years and there was practically an equal number of each sex Many pupils took only part of the tests because they were not all given the same day. Thus, the number of subjects per test

is large in comparison with the number that took the entire series. The number for each age taking all 11 tests averaged only 22, while the average number of like age for any one test was 452

In order to test as many subjects as possible, it was found advisable to give the first seven tests in the following battery as group tests To insure against any group influence, great care was taken to see that each pupil kept his work covered, no communication was allowed, and all subjects from any one school were given the same test on the same day The last four tests were given individually. The card was also included as an individual test for those who could not write The nature of these tests was never divulged to any of the subjects The majority considered them a new and peculiar series of "nut tests"

The tests are described in the order in which they were given.

1. *The trap lines* were devised by Binet (3, pp 87-109) and the material and procedure are identical with his A series of 12 lines drawn on a white paper were shown one at a time for 7 seconds. The first line is 12 mm. long and each of the next 4 increases 12 mm. in length as do numbers 7, 9, and 11 Series 6, 8, 10, and 12 are traps in that they are the same length as the line immediately preceding. The subjects were asked to look at the line exposed and to make one of exactly the same length on a paper furnished them, placing the first under the second and so on. The scoring differs from Binet's. Marking on the basis of susceptibility to suggestion, those subjects are most suggestible who increase the length of the trap lines as they do for the others For each trap line increased, a score of 2 is given as the suggestibility index. This allows for a range of scoring at intervals of 2 from 0 through 10.

Many of the younger children barely looked at the last six lines, marking them all of progressively increasing length One might say from this that expectant attention increases the degree of suggestibility of a person

2. *Progressive increase in the length of lines,* a modified Binet test (3, pp. 110-160), consists of 10 parallel lines which start at varying distances from the margin. The first is 12 mm. long, the next 4 increase progressively (12 mm.), while the last 5 are all equal in length. The procedure and scoring are identical with those for the trap lines. For each of the last five lines drawn longer than the preceding one, a score of 2 is given.

3. *Simple aesthetic preference* is based on a series by Brown (4),

comparing the proportions of a rectangle, a cross, and a square drawn by the subject as he prefers to those drawn by him after suggestions of improper proportions are made. With the youngest children, drawings were made on the blackboard to illustrate a rectangle and a square. Three figures were drawn for a rectangle: one almost square, one longer than it was wide, and one higher than it was wide Three figures were likewise made for a cross: one with the cross piece near the top, one with the piece near the middle, and one with the cross piece near the bottom. The subjects were then asked to draw six figures. The explanation given before each drawing was

A (1) "A man is going to put a 'for sale' sign on his house. I want you to draw the *shape* of the 'for sale' sign you would like best to have him use"

(2) "Now just under this 'for sale' draw the kind of a rectangle you like best" Note: "Most people like best to draw their rectangles higher than they are wide"

B (1) "I want you to draw the kind of a cross you would like best to see on a soldier's grave."

(2) "Now just under the soldier's cross draw the kind of a cross *you* like best" Note. "Most people like best to put the cross-piece nearer the bottom than the top."

C. (1) "A man is going to buy a new picture to hang on the wall in his home I want you to draw the shape of the picture (the shape of the frame) you would like best to have him buy."

(2) "Now just under the picture draw the kind of a rectangle you like best." Note. "Most people like best to draw theirs almost square"

For no yielding to the suggestion, a score of 0 is given. One figure showing suggestion gives a score of 3, two a score of 7, and 3 a score of 10 One would expect more subjects to yield to the suggestions for the rectangle than for the cross because of the uncommon and inartistic method suggested for the cross. This was found to be the case Most of the "for sale" signs were drawn a bit longer than wide.

4. *Directive suggestion regarding the length of lines* is one of Binet's tests (3, pp. 225-227), but 11 rather than his 36 lines, each 60 mm in length, were shown one at a time. The subjects were to reproduce them as for the first two tests The first line was exposed without comment, but an explanation of "Here is one that is longer," accompanied the second and "Here is one that is shorter,"

the third These suggestions continued alternately through the series. A score of 1 is counted for every suggestion taken. Many of the youngest children accepted the suggestions without question, giving only slight attention to the lines With others, there was a conflict between the verbal suggestion given and the impression received by observation If the conflict was great enough, the prestige suggestion was overruled and the line drawn correctly.

5. *Illusion of smell* is a modified form of Slosson's experiment (18) A small bottle of water was wrapped in cotton and placed in a box While carefully unwrapping and holding it up, the examiner explained: "In this bottle there is something that I think you have never smelled before. I am going to pour some on a piece of cotton, hold it in the air, and begin counting one count a second. As you smell it, you are to put the number you hear me say on your paper. Remember, all you need is one number, the number you hear me say when you first smell this." Examiner poured some water on the cotton, holding her head away as if the odor were disagreeable, and counted to 100. This test is scored by the all-or-none method The index of suggestibility is +10 if a number is recorded, 0 if no number is written or a statement to the effect of smelling nothing, and —10 if the validity of the test is challenged. There were few who denied the validity. The youngest smelled an odor on the first few counts and as the age increased the recorded number tended to be higher Those of high-school age were the only ones who pronounced it a fake. A very few in some of the lower grades hastily wrote a number after the test was completed Their papers were collected separately and not counted. These numbers tended to be neither small nor particularly large.

6. *The card* was first used by Binet (3, pp. 244-324) and involved fidelity of report as affected by leading questions. The card was deep yellow in color, 22 cm wide and 15 5 cm long, and had glued to it six objects: a button, a penny, a violet brown unused stamp, the picture of a man's head (hatless), the picture of a crowd (both of these were without color), a light green store ticket (printing, B. *Marsh & Company, Linens and Waists, $6 75*). The card was exposed for 12 seconds and the subjects then wrote the answers to 15 suggestive statements. Briefly, they are

1. What color is the thread that goes through the four buttons fastening them to the card?
2. Draw the place the button has been broken.

3. *Is the picture of the man dark blue or brown?*

4. Is the gentleman's left leg crossed over his right leg or his right leg crossed over his left leg?

5 Draw the shape of the hat on his head.

6. What is he holding in his right hand?

7 *Where is the small hole in the penny?*

8. What city is on the postmark for the stamp (postmark explained)

9. Is the stamp light red or dark red?

10. Draw the thread which fastens the ticket to the card.

11. Is the ticket light green or dark green?

12 *In the picture of the crowd, where is the little dog?* (*none* there)

13. How is the man dressed who is being arrested by the police? (no arrest)

14 The seventh thing on the card is a picture What is it *about?*

15. What is the eighth thing on the card?

An object described on the card was counted 1 and an object not on the card was counted 2 Zero was given for no response or an answer inferring loss of memory, while a minus score was given *for flat denial of the validity of the question*

7. *Illusion of sight* is after Small (19) but slightly modified. A small iron animal, a deer, has a cord around its neck which is attached to a stick of wood on the inside of a box. The thread from a ball of cord hidden in the box is wound twice around the stick to furnish sufficient friction to make it taut. It comes through a hole *in the lid and is wound on a windlass which is attached to the* top of the box. The subjects are supposed to see the animal move when the windlass is slowly turned. The subjects were asked to look at the nose on the deer while E turned the windlass and counted to 100 The procedure and scoring are similar to the illusion of smell. *As with the illusion of smell the younger children* saw a movement on the first few counts Some put down a number after the test was completed as with the test for smell Over half of these gave the same response on the former test. Both in the junior and senior high schools the validity of the test was questioned.

8. *Directive suggestion regarding colors* has been used by Maxfield As a preliminary check on color-blindness, no subject was

used for this or the following test who could not correctly name each of the six colors From 15 one-inch squares of each of the following colors red, orange, yellow, green, blue, and violet, the subject was first asked to hand E 3 red, 1 green, and 2 blue squares. He was then asked how many of each of the three colors he took up. E next asked for 1 yellow, 3 violet, and 2 green, and questioned as to the number of each picked up. Lastly, E requested 1 red, 1 violet, and 2 *yellow* Subject was then asked the number of violet, red, and *orange* used If subject said that none were orange, examiner asked how many yellow ones were used The score is either 10, indicating the number of orange, or 0. Those who accurately remembered the number of colors chosen proved the most resistant to suggestion, indicating that accuracy of observation and good memory are important factors in influencing resistance to suggestion Many subjects replied with the number almost at the same time that the stimulus "orange" was given The older the subjects, the more correct were the responses

9. *Contradictory suggestion regarding color* is taken from Binet (3, pp 211-218). Nine small white cards were wound with the following colors of silk floss· pure blue, gray-blue, green-blue, blue-green, yellow-green or moss green, still more yellow, olive green, yellow with a green tint, and golden yellow The subject was asked to name each color so the examiner could hear and then to write its name on paper. Whenever a verbal response other than blue was given by the subject, the examiner said in monotonous tone, "No, blue." A score of 1 was given for 1 incorrect blue response, 2 for 2 incorrect blues, 4 for 3 incorrect blues, 5 for 4, 6 for 5, 8 for 6, and 10 for 7 The younger children accepted the suggestion without question, some older pupils seemed dubious of the correction but recorded the suggestion, some writing blue and at the same time saying the correct color. The high-school students, however, took it as a huge joke or assumed a contemptuous air After the first or second suggestion of blue for green, the majority of children under high-school age called all the other colors blue spontaneously

10. *Contradictory suggestion regarding the length of lines* is after Binet (3, pp. 219-225) A series of 24 numbered lines, the length of the shortest being 12 mm and each succeeding line progressively lengthening by 4 mm., was shown to the subject He was to determine which of the series was comparable in length to three other lines exposed one at a time. The first of the second group

is 32 mm long and equal to line 6 of the first series, the second of the group of three is 56 mm and equal to line 12, and the third is 80 mm. and equal to line 18. A line of the second is exposed for five seconds after which he chooses that in the first series which is its equal in length No matter what his choice, the examiner points to the line immediately below, saying, "Is it not rather line—?" If subject agrees, examiner points to the next below repeating, "Is it not rather line—?" For complete resistance a score of 0 was obtained, acceptance of all six suggestions counted 10, five counted 8, four counted 6, three counted 5, two counted 3, and one counted 1 The younger children readily accepted the suggestions without question Those making accurate first judgments tended to be most sure of their own choices Here accuracy of observation seems to accentuate resistance to suggestion

11 *Progressively increasing weights* is a modified Binet (3, pp. 110-208) test Ten weighted cubes of equal size were placed before the subject for him to determine by lifting whether each succeeding cube was lighter, heavier, or the same as the one immediately preceding. The first was 20 grams and Nos 2 through 5 progressively increased by 20 grams The last five were all 100-gram weights. For each of the last five weights judged heavier, a score of 2 is given Many of the younger children failed to judge the first five weights correctly They were under the impression, however, that no two weights in succession were the same Many that judged each of the last five progressively heavier raced through them without due consideration, the idea of progressive increase was so firmly fixed. The majority of those who gave accurate judgments for all of the last five weights were slow and deliberate in their procedure

TABLE 1
SHOWING THE FREQUENCY WITH WHICH A PARTICULAR SCORE OR RANGE OF SCORES MAY OCCUR ON ALL ELEVEN TESTS

Score or score range	*Number of tests yielding this score or score range*	*Percentage of tests yielding this score or score range*
10	11	100
9-8	9	81 8
7-6	10	90 9
5-4	10	90 9
3-2-1	11	100
0	11	100
Minus	3	2 72

Thus it would seem that resistance to suggestion depends to a considerable extent upon accuracy of observation

The scoring system is so devised that all the tests are made to yield closely comparable scores on a scale which does not exceed 10 points on either the plus or minus side of 0 or a neutral point Three of the eleven tests allow for only gross scores of marked positive, neutral, or marked negative responsiveness, while the other eight show varying steps or degrees of difference in suggestibility Table 1 indicates the frequency with which any particular score or range of scores may occur on all eleven tests It is therefore possible to obtain only one of six score ranges or steps on almost all of the tests while a negative scoring occurs very infrequently

Results

Table 2 and Figure 1 give the average suggestibility scores by age and sex for all 11 tests. For both boys and girls the highest score obtained was at 7 years The smaller suggestibility index at year 6 indicates a trend toward lessened suggestibility in the younger

TABLE 2

Average Responsiveness of School Children to Eleven Forms of Suggestion as Related to Sex and Age

Ages	Sex: Boys	Sex: Girls
6	6 82	7 87
7	8 41	8 26
8	7 17	7.58
9	6 58	6 88
10	5 79	6.19
11	5 68	5 19
12	4 36	4 63
13	3 58	3.70
14	2 29	2 05
15	1 73	2 17
16	1 53	1 78
Mean	4.903	5 118
Diff between sex means	215	
$P E_D$	079	
$\frac{D}{P E_D}$	2 7	

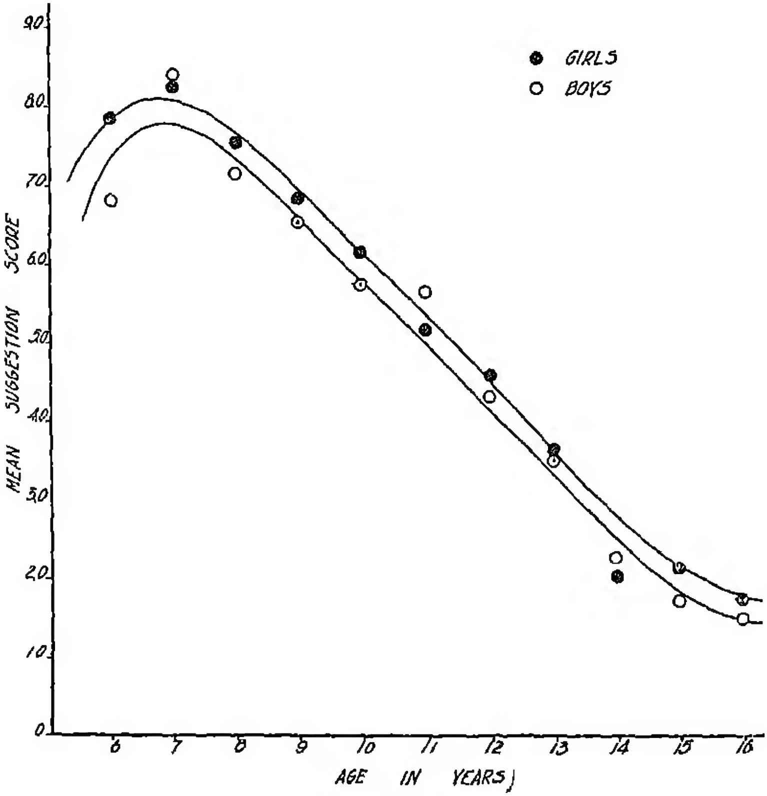

FIGURE 1
COMPOSITE GRAPH SHOWING RESPONSIVENESS OF CHILDREN TO ELEVEN FORMS OF INDIRECT SUGGESTION AS A FUNCTION OF AGE AND SEX

children. Unfortunately, ages were not extended low enough with the first study group to note any very significant decrease. After 7 years suggestibility indices lessen regularly with the older age groups. To put it another way, after 7 years suggestibility decreases with increase in age In this respect the results of the first study agree with those of previous investigators.

Differences were obtained between the scoring of the boys and the girls. At only 3 of the 11 ages do the boys exceed the girls in average suggestibility At the other 8 ages, or 73%, the girls exceed the boys. For all ages taken together, the girls average .21

of a point more suggestible, yielding a critical ratio of 2.7. This critical ratio does not indicate a very reliable difference between the sex means. Although the difference is not very significant, the slightly higher suggestibility of girls agrees with the conclusions of the majority of experimenters.

Do subjects differ in susceptibility to suggestion from one situation to another or are they equally suggestible for all? To determine whether or not subjects showed the same degree of suggestibility for all tests, the number and percentage of each of the 7 possible score ranges received on all 11 tests was computed for each subject. Table 3 gives the percentage of consistency scores for all ages when

TABLE 3
PERCENTAGE OF CONSISTENCY WITH WHICH EACH SUBJECT RECEIVED THE SAME SCORE ON ALL ELEVEN TESTS AS RELATED TO AGE AND SEX

		Percentage of consistency score						
Age in years	Sex	10	9-8	7-6	5-4	3-2-1	0	Minus
6, 7, 8	Boys	56	8	9	7	11	8	1
	Girls	56	12	9	6	9	8	
9, 10, 11	Boys	29	11	12	9	21	15	2
	Girls	31	9	10	13	17	14	6
12, 13	Boys	18	6	7	10	28	29	2
	Girls	20	8	11	12	22	25	2
14, 15, 16	Boys	6	7	5	10	19	49	4
	Girls	13	3	6	7	18	45	8

conveniently grouped to bring out general trends, that is, ages 6, 7, and 8 showed a similar trend in scoring as did 9, 10, and 11; 12 and 13, 14, 15, and 16. Table 4 and Figure 2 show the highest percentage of consistency scoring for each age of both boys and girls. Just a little over 1/2 the children from 6 through 8 received the same score on all 11 tests, between 1/3 and 1/4 from 9 through 13, and less than 1/2 from 14 through 16. It is very interesting to note that at the ages where the children are most suggestible and least suggestible there is more consistency in scoring. Therefore, those children who show the greatest degree of suggestibility for one situation tend to be generally more suggestible, whereas those least suggestible or contra-suggestible tend to remain so in different situations. According to Table 4, the boys exceed the girls at 5 different ages and the girls likewise exceed the boys at 5 different ages. The

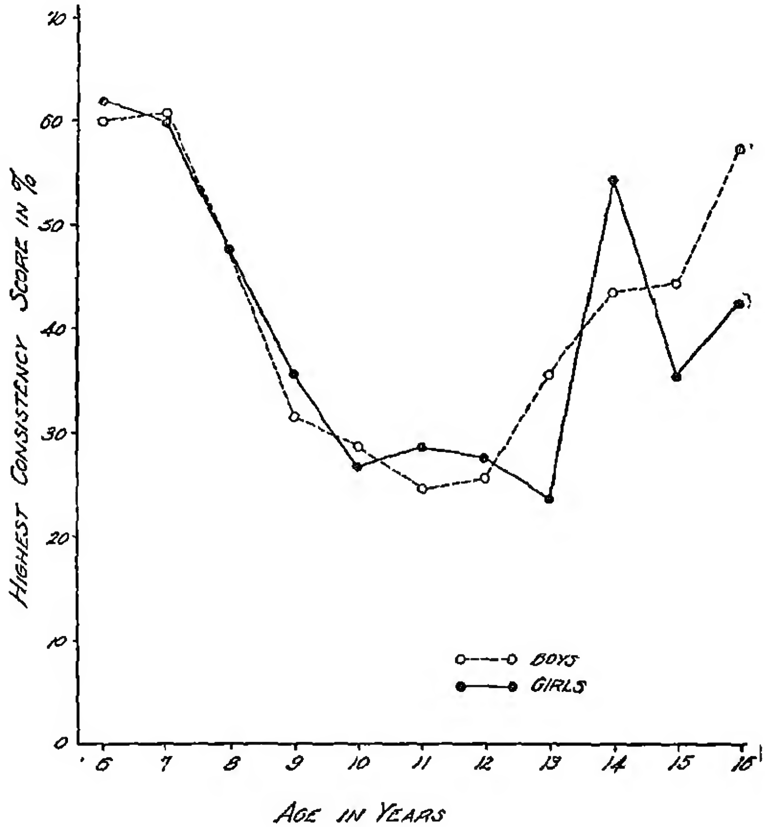

FIGURE 2
COMPOSITE GRAPH SHOWING HIGHEST PERCENTAGE OF CONSISTENCY SCORE BY AGE AND SEX ON ALL ELEVEN TESTS

difference between the sex means is 1 455 in favor of the boys, yielding a critical ratio of 0 97

Using the U-curve (found by Aveling and Hargreaves and Estabrooks to be typical of test results where the prestige factor is operative) as a criterion of prestige suggestion, the scores obtained for each test were examined and curves plotted to note evidence of prestige Only 3 of the 11 tests give any evidence of prestige as judged by the U-curve They are directive suggestion in regard to the

TABLE 4
HIGHEST PERCENTAGE OF CONSISTENCY SCORING ON ALL ELEVEN TESTS BY AGE AND SEX

Age	Sex	
	Boys	Girls
6	60	62
7	61	60
8	48	48
9	32	36
10	29	27
11	25	29
12	26	28
13	36	24
14	44	55
15	45	36
16	58	43
Mean	42.182	40.727
Diff. between sex means	1.455	
$P.E._D$	1.493	
$\frac{D}{P.E._D}$	0.97	

length of lines, contradictory suggestion in regard to colors, and contradictory suggestion in regard to the length of lines

Although prestige cannot be noted to operate for any of the age groups in directive suggestion in regard to the length of lines, the nature of the scoring for different ages is such that it is characteristic of the test when the ratings for all the ages are taken together The high degree of suggestibility of the children from 6 through 11 and *the non-suggestibility of the children from 12 through 16 make a* well-defined U-curve. Only the 6- through 8-year-olds show any prestige influence for contradictory suggestion in regard to the length of lines, all the other age groups being so little influenced by E's statements as to overbalance the suggestibility of the younger children when ratings of all the ages are averaged. Contradictory suggestion in regard to colors shows a distinct prestige factor operating for the test as a whole and quite characteristic of those from 9 through 13. There is a faint impression of prestige in the 6- through 8-year-olds but it is not definite enough to be considered as such The youngest children were inclined to accept E's contradiction of facts unques-

tioningly, those from 9 through 13 were about evenly divided, some accepting E's correction, others holding to their original estimates, whereas the oldest, from 14 through 16, gave little credence to E. The high degree of suggestion for the youngest and the non-suggestion of the oldest only accentuate the U-trend for the middle ages, yielding a definite U for the test as a whole

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Department of Child Welfare
White Plains, New York

LA SUGGESTIBILITÉ DES GARÇONS ET DES FILLES ENTRE LES ÂGES DE SIX ET DE SEIZE ANS

(Résumé)

On a fait subir onze différents tests de suggestion à des écoliers des écoles municipales de chaque sexe et à chaque âge de 6 à 16 ans Ceux qui on subi toute la série ont été en moyenne 22 à chaque âge tandis que le nombre moyen des sujets de même âge pour un seul test a été de 452 On a employé les résultats du même nombre de garçons et de filles

On a fait la moyenne des indices de suggestibilité pour chaque sexe et chaque âge Les filles ont eu un peu plus de suggestibilité que les garçons Les garçons possèdent en moyenne plus de suggestibilité seulement à 3 des 11 âges étudiés. Aux autres 8 âges, ou 73%, les filles possèdent plus de suggestibilité que les garçons La différence moyenne entre les sexes donne une proportion critique de 2,7, ce qui signifie qu'on obtiendrait au hasard une différence de cette grandeur seulement une fois sur trente Après 7 ans, la suggestibilité a montré une décroissance régulière avec l'accroît de l'âge. Les enfants à l'âge de 16 ans n'ont possédé que le sixième de la suggestibilité de ceux âgés de 7 ans Une tendance à moins de suggestibilité chez les très jeunes enfants est indiqué par un plus petit index de suggestibilité à l'âge de 6 ans Quelques-uns des test montrent les résultats maxima à 8 ans au lieu de 7 Cette irrégularité est probablement plutôt une différence dans les caractéristiques des formes de suggestion qu'un résultat du hasard Les sujets n'ont tendu que rarement à montrer le même degré de suggestibilité pour tous les tests Il est intéressant de noter cependant qu'aux âges où les enfants possèdent le plus de suggestibilité (6-8) et le moins (14-10) les résultats sont les plus constants.

MESSERSCHMIDT

DIE EMPFANGLICHKEIT FÜR SUGGESTIONEN VON KNABEN UND *MADCHEN IM ALTER VON SECHS BIS SECHSZEHN JAHREN*

(Referat)

Man gab Kindern offentlicher Schulen, beider Geschlechter und jeden Alters von sechs bis sechzehn Jahren, elf verschiedene Suggestionsteste Die durchschnittliche Zahl der Kinder, die sich der ganzen Testserie unterzogen, betrug 32 fur jedes Altersjahr, wahrenddem die durchschnittliche Kinderzahl derselben Alter fur irgend einen der Teste 452 war. Man benutzte die Angaben einer je gleichen Zahl von Knaben and Madchen

Der Durchschnitt der Empfanglichkeitsangaben wurde fur jedes Alter und jedes Geschlecht berechnet Die Madchen waren ganz wenig empfanglicher als die Knaben Nur in 3 von den 11 untersuchten Altersjahren erweisen die Durchschnitte die Knaben als empfanglicher als die Madchen *In den ubrigen 8, oder also in 73%, der verschiedenen Altersjahre* ubertreffen die Madchen die Knaben Die mittlere (mean) Differenz zwischen den *Geschlechtern ergibt ein kritisches Verhaltnis* von 2,7, was bedeutet, dass man einen Unterschied solcher Grosse nur in einem aus dreissig Fallen durch Zufall erhielte Nach dem siebenten Altersjahr vermindert sich die Empfanglichkeit regelmassig mit zunehmendem Alter. Kinder im sechzehnten Altersjahr waren nur ein Sechstel so empfanglich wie Siebenjahrige Eine Tendenz nach verringerter Empfanglichkeit bei sehr jungen Kindern wird ausgedruckt durch einen kleinern Empfanglichkeitskoeffizienten im sechsten Altersjahr Einige Teste ergeben Maxima beim achten statt beim siebten Altersjahr Diese Unregelmassigkeit ist wohl eher auf einen Unterschied *in den Merkmalen* der *Suggestionsformen* als auf Zufall zuruckzufuhren. Die einzelnen Versuchspersonen ziegen wenig Tendenz auf alle Teste *mit demselben Grad der Empfanglichkeit zu reagieren. Es ist* interessant, dass sich bei den Altersjahren, in denen die Kinder am empfanglichsten (6-8) und am wenigsten empfanglich (14-10) sind, die grosste Gleichformigkeit der Angaben feststellen lasst.

MESSERSCHMIDT

SHORT ARTICLES AND NOTES

THE EXPECTED AVERAGE DIFFERENCE BETWEEN INDIVIDUALS PAIRED AT RANDOM[1]

QUINN MCNEMAR

In the statistical treatment and interpretation of certain data it sometimes seems desirable to supplement or replace correlational techniques by the use of some inverse measure of relationship One such measure which has been used is the average difference between the individuals of pairs, as, for example, the difference between friends, or between husband and wife, or between twins, etc In order to interpret such a measure, it is necessary to know something about the theoretically expected difference for individuals paired at random Certain investigators have attempted to arrive at this theoretical value by drawing pairs at random and then computing the *average difference between such pairs This method is not only laborious* but also lacking in precision because of the sampling errors involved in the drawings It is the purpose of this note to give two proofs that the theoretically expected average difference between individuals paired at random is $2\sigma/\sqrt{\pi}$, in which σ is the standard deviation for a given trait for a given sample

The writer's first attack on the problem was to build up a distribution of the differences between a million pairs of individuals paired by use of elementary principles of probability rather than by random drawings The procedure was as follows The base line of the normal probability function was divided into 33 segments of $.3\sigma$ each (sufficient range to include a million cases) By reference to a table of the normal probability function it is possible to state the probability of drawing an individual from any given segment Let P_i represent this probability. Let the probability of drawing the second individual of a pair from any given segment be P_j (all drawings on the assumption of an infinite population) By the multiplication theorem, P_iP_j is the probability of drawing a pair who show a difference in sigma units equal to the distance from the midpoint of the i-th interval to the midpoint of the j-th interval Thus by letting i and j take successive values it is possible to build up a distribution of differences for individuals paired at random which will approximate very closely the theo-

[1]The value given in this note was derived and used in connection with a study on twin resemblances in motor skills Since its derivation, the writer has found that the same value has already been given by J O Irwin (1, p 101) Thus a rediscovery has been made, and the result is being published so as to make it more generally known among psychologists

retical distribution of an infinite number of differences Straightforward computation of the average difference for a million pairs so selected gave the value 1 12837929+ times sigma Since the value of $2/\sqrt{\pi}$ is 1 12837916+, it was inferred that the theoretically expected average difference between individuals paired at random is $2\sigma/\sqrt{\pi}$.

The second proof is based on the theorem that for uncorrelated measures the variance of a difference is equal to the sum of the separate variances, i.e., $\sigma^2_d=\sigma^2_1+\sigma^2_2$ Since in the present case the individuals are drawn from the same parent population, $\sigma^2_1=\sigma^2_2$, so that the variance of the difference between individuals paired at random is $\sigma^2_d=2\sigma^2$. Now the average difference between the pairs will correspond to the mean or average deviation of the distribution of differences, and since the average deviation of any normal distribution equals $\sqrt{2/\pi}$ times its sigma, it follows that $A.D._d= \sigma_d\sqrt{2/\pi}=2\sigma/\sqrt{\pi}$

A word should be said concerning the σ value which should be used in determining the theoretically expected average difference in the practical situation Since our purpose has been to answer the question, what would be the average difference if the individuals of a given group were paired at random, it would seem best to state this expected value in terms of the standard deviation of the group under consideration But it should be noted that if one wishes to infer from the σ of one group what the expected average difference would be for individuals paired at random from another sample an error may be introduced because of the fluctuations in the standard deviations due to sampling If the experimental group is small and if a σ is available from a large sample similarly selected, it would be better to use the σ based on the larger sample.

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Stanford University
California

THE WHOLE-PART PROBLEM IN MEMORIZING POETRY

GRACE O MCGEOCH

In a recent paper (3) on the whole-part problem in memorizing meaningful material the writer called attention to the need of a revaluation of the experimental data in view of contemporary textbook treatment of the whole-part issue Quotations were given from five recent texts in educational psychology which state explicitly that the whole method is superior to the part in both learning and retention in spite of the fact that the experimental

data at present do not justify any answer to the question Is the whole method superior to the part? Attention was called to the fact that the question so framed is scientifically unanswerable No method can be inherently or absolutely the best method The whole-part problem, however, conceived in terms of the conditions under which one method of memorizing is superior to another is amenable to a scientific answer, which, however, awaits much further experimentation A previous analysis (1) of the experimental data had suggested that the relative efficiencies of the whole and part methods in learning and retention are functions of, at least, seven main factors material, subjects, practice, form of the part method used, methods of measuring efficiency of learning and of retention, and the length of the interval. It was concluded that the available data do not justify any generalizations regarding the reciprocal and differential effects of the above factors, much less the recommendation for classroom use of any particular method of memorizing

Experiments on memorizing meaningful material are of more immediate interest to the educator than those in which nonsense syllables or mazes are used for learning materials The number of investigations, however, on memorizing poetry which have yielded results amenable to statistical *analysis have been surprisingly few. Reed (5), and more recently Stroud and Ridgeway (7), used college students as subjects under conditions of* learning to complete mastery with massed practice Reed concluded that, although there were no reliable differences in learning between the three methods employed in the mean time scores, the majority of his subjects found the whole method inferior to the part methods Stroud and Ridgeway also concluded that the whole method was less economical in learning than the progressive part and part methods in terms of trials, which constituted their only valid measure of learning efficiency, but that there were no significant differences between the three methods in retention after an interval of one week It is unfortunate in the interest of further experimentation on the whole-part problem designed to isolate out the factors of which the relative efficiencies of the learning methods are functions that the poems used as *learning materials in these two investigations* were not described more completely and that the subjects' habitual methods of memorizing were not considered.

Winch (8), also using massed practice but a time-limit, not a work-limit technique, found, with school children between the ages of 11½ and 13, the whole method to be inferior to the progressive part in learning "short" poems, but no valid difference between the two methods in the learning of a "long" poem under conditions of distributed practice. His results, however, must be considered in the light of the fact that the majority of his subjects had habitually employed a verse-by-verse method of memorizing, and also that learning efficiency was measured in terms of the number of correct words, not lines

The writer (2), likewise using massed practice and the time-limit technique with children, found no reliable differences between the whole, progressive part, and pure part methods in the learning, or in the retention after a 24-hour interval, of 12 lines of poetry. The poetry units in this experiment were taken from two poems by William Cullen Bryant, "An Autumn Walk" and "A Life Time," which, although of relatively simple meter and rhyme scheme, were somewhat abstract in content and lacked the obvious continuity and appeal of narration. It was suggested that the lack of differences between the three learning methods might have been, in part at least, a function of two factors: (1) the type of learning material, and (2) the previous practice with the different methods of learning. It was possible to make a rough test of the first hypothesis by analysis of data at hand secured in an investigation on reminiscence (4), in which the experimental conditions were sufficiently similar to justify comparisons of results.

In this experiment only the whole method and the pure part method were used. The subjects were all the nine-, ten-, and eleven-year-old children in six public schools, four in Fayetteville, and two in Springdale, Arkansas. Half the subjects learned by the whole method and half by the part. Factors of age, sex, grade, and type of school were taken into account when dividing the subjects into the two groups. From 20 to 40 children were tested at one time. All testing was done in the schoolroom during school hours. The poem learned, Mary Howitt's "The Spider and the Fly," is of the ballad type and is considered a children's poem. Stanzas 4 through 8 (24 lines) were arranged in 12 double lines which were printed with 1 inch between each line. This arrangement facilitated the mechanical requirements of learning by the part method, in which a blank sheet of paper, which was used to cover the poem, was slipped down a line at a time until all 12 lines had been exposed. After each line had been read aloud once by the experimenter, the subjects were given 20 seconds in which to study it. This procedure took 4 of the 5 minutes allotted to learning. The last minute was devoted to a review of all 12 lines. For the whole method, the poetry was printed with a single space between each line. The experimenter first read aloud the material once, and then 5 minutes were given in which to read all the way through the poem as many times as possible. With both learning methods, immediately after the learning period the children were given all the time they wanted for a written recall of the poem. They were instructed to try to recall the exact words of each line if possible, otherwise, to give the thought of the line. The subjects did not know that they would be given a retention test after an interval of 24 hours. Two types of scores were obtained: V-scores, the number of lines reproduced *verbatim*, and S-scores, the number of lines reproduced as to sense added to the V-scores.[1] A line was credited irrespective of relative or absolute position.

[1]The reason the S-scores were not treated separately but included the V-scores was the fact that we are primarily interested in the total number of

Each line, since it contained two lines as the poem is usually printed, was treated and scored as two

The records of only 310 (70%) children were used, although 440 completed the experiment, since it was discovered by questioning the children after the retention test that 130 (30%) had heard or learned the poem or part of it prior to the experiment These 310 subjects were distributed as follows with the whole method, there were 38 nine-, 62 ten-, and 61 eleven-year-old children; while with the part method the numbers were 35, 60, and 54 for the three ages respectively Since separate analyses of the data on the basis of age and sex revealed no reliable or consistent differences between learning methods, results with the three ages and two sexes were combined These are summarized in Table 1 which gives, for each learning method, the mean score and the sigma of the mean for both V- and S-scores in learning and retention, the percentages of retention, and the differences with their sigmas

TABLE 1

N	Method	Learning V-scores Mean σ_M	Learning S-scores Mean σ_M	Retention V-scores Mean σ_M	Retention S-scores Mean σ_M	Percentage of retention V	Percentage of retention S
161	Whole	2 04$\pm$ 19	8 62$\pm$ 35	1 71$\pm$ 17	8 45$\pm$ 35	84	98
149	Part	2 50$\pm$.22	8.62$\pm$ 35	2 11$\pm$ 23	7 55$\pm$.36	84	87
D and σ_D		—46$\pm$ 29	0 $\pm$49	—40$\pm$ 29	90$\pm$.50	0	11

Inspection of Table 1 shows that there are no reliable differences between the whole method and the part method with either type of score in learning or retention Furthermore, the apparent difference of 11% in percentage of retention with the S-scores is not valid since the difference in retention is not reliable On the basis of these results the conclusion can be drawn that with poetry which is appealing to children there are no significant differences between the whole method and the part In so far as this investigation is similar enough to justify comparison, it can be concluded that, with the pattern of experimental conditions used, the lack of valid superiority of either method is not a function of the type of poetry learned [2]

lines the subjects learned That total amount can be measured by the more severe criterion of *verbatim* reproduction or by the more lenient criterion of reproduction of the sense of the line The latter criterion necessarily includes the former

[2]From a study of the following summary of the experimental conditions in the two experiments, it is evident that, in spite of minor differences, in general the similarity is sufficient to warrant comparison The conditions of the study reported in (2) are listed under A, while those of the present

The second hypothesis, suggested above (2, p. 357), was that the practice factor might acocunt, in part at least, for the results obtained The factor of practice includes not only the methods of memorizing which the children had been using prior to the experiment but also the novelty of the imposed formal part methods involved in the experimental procedure. It is highly doubtful that a child would spontaneously use a part method in which, during the learning time allotted, each line, the last as well as the first, would receive the same length of time for study

In an attempt to test the practice factor another experiment was conducted. The classroom teachers of the third and fourth grades in five public schools in Columbia, Missouri, were asked what methods of memorizing they had their pupils use. Out of the ten grades, a form of the progressive part method was taught by six teachers, the whole method by two, while two teachers permitted the children to use their own methods In the discussion, it became evident, however, that the habitual methods employed by the pupils were not necessarily the methods recommended It was decided, therefore, that the best procedure to obtain results with the methods that had had the most previous consistent practice was to let the children spontaneously select their own methods of learning, and, after the experiment was over, find out what methods had been used Attention might be called here to the fact that the use of children in higher grades would not necessarily have insured more firmly established learning methods because of the lack of uniformity among the teachers in the method recommended

Since it was impossible, because of practical difficulties, to secure all the nine-, ten-, and eleven-year-old children in the five schools used, only the children of these three ages who were in the third and fourth grades served as subjects The first six stanzas (24 lines) of Mary Howitt's poem, "The Spider and the Fly," constituted the learning material Each child received a copy of the poem printed on thin cardboard After the experi-

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investigation are listed under B The writer conducted both experiments and scored all the papers

Subjects (A) 62 nine- and ten-year-old school children of average and very superior intelligence, (B) 310 nine-, ten-, and eleven-year-old school children of all intelligence levels for each age found in the public schools

Methods (A) With the whole method, 6 minutes for reading through the poem as many times as possible, while with the pure part method, 20 seconds for each line followed by 2 minutes for review of the whole poem, (B) with the whole method, a single reading aloud of the whole poem by the experimenter, followed by 5 minutes for reading through the whole poem as many times as possible, while with the pure part method, after a single reading aloud of each line by the experimenter, 20 seconds for study of each line, followed by 1 minute for review of the whole poem

Procedure General procedure, including written recall immediately after learning, retention test after an interval of 24 hours, and method of scoring, the same for both experiments.

menter had read it aloud once, five minutes were given for learning Nothing was said about the method of learning The children were simply told to memorize as much of the poem as they could in the allotted time Immediately after the learning period, a written recall was taken, for which there was no time limit The children were instructed to reproduce the exact words of each line if possible, otherwise, the thought. They were not told that there would be a retention test after an interval of 24 hours. The retention test was therefore unexpected As each child finished writing this second recall he laid his paper aside and turned to his own school work until all the children had finished The experimenter then asked the subjects how they had learned the poem The whole method was carefully explained and also that verse-by-verse learning meant completely learning each verse before beginning the next verse, and that line-by-line learning meant literally learning one line at a time The difference between a pure part method and a progressive part method was explained, but as far as the experimenter could discover from the children's own description of their methods, no subject had employed a pure part method. Those children who were not certain of the method they had used were questioned individually by the experimenter After each subject had written on his paper the method employed in learning the poem, he then indicated his habitual method of memorizing Again individual attention was given to those who were uncertain. There were three types of methods listed reading through the whole poem again and again, learning it verse by verse, and learning it line by line

Although 238 children completed the experiment, the records of only 172 (72%) were used The papers of the following subjects were discarded those who had learned in units larger than one line or less than four lines (four lines to a verse), those who did not know definitely how they had learned the poem, the few whose habitual learning methods differed from the methods used in the experiment; those who had read or learned the poem or any part of it prior to the experiment, and the few who had happened to see the poem in books at home during the 24-hour interval All the papers were marked by the experimenter who used the same system of scoring as that used in the two studies discussed above

The total of the 172 subjects was distributed as follows according to learning method and age. whole method, 6, 0, and 3 for ages nine, ten, and eleven respectively; verse-by-verse learning, 66, 17, and 11, and line-by-line learning, 43, 20, and 6 It is interesting to note, and unfortunate for the purposes of our study, that only 9 (5%) of the children spontaneously and habitually used the whole method Since the small number of cases precludes statistical analysis and, consequently, comparisons with the other two methods, only the average scores and the ranges for this method are given below in Table 2. Separate analyses of the data on learning by verses and

by lines on the basis of age, sex, and grade revealed no reliable or consistent method differences dependent upon any one of these factors. The results, therefore, with the three ages, two sexes, and two grades combined are presented in Table 2, which includes for both V- and S-scores the mean score and the sigma of the mean for learning and retention, the differences between methods with their sigmas, the percentages of retention and their differences.

TABLE 2

		Learning				Retention				Percentage of retention	
		V-scores		S-scores		V-scores		S-scores			
N	Method	Mean	Range	Mean	Range	Mean	Range	Mean	Range	V	S
9	Whole	1.44	0-4	4.66	2-8	1.88	0-5	6.00	2-11	131	129
		Mean	σ_M	Mean	σ_M	Mean	σ_M	Mean	σ_M	V	S
94	By verses	2.47	± .19	7.27	± .39	2.42	± .23	7.34	± .42	98	101
69	By lines	2.30	± .21	6.24	± .40	2.40	± .23	6.58	± .42	104	105
D and σ_D		.17	± .28	1.03	± .56	.02	± .32	.76	± .59	—6	—4

Inspection of Table 2 shows that all the differences between verse-by-verse learning and line-by-line learning, with the exception of those in the percentages of retention, are in favor of the former. The size of the sigmas of these differences, however, renders them all unreliable. Consequently, the differences in percentage of retention are likewise not valid. On the basis of these results it can be concluded that there are no significant differences between the two spontaneously and habitually employed methods of memorizing, two progressive part methods, one involving units of a single line in length, and the other units four lines in length.

This lack of differences might be wholly a function of the too great similarity of the methods in which the units differed by only three lines. It is unfortunate that the whole method was not used by a sufficient number of children to justify comparisons between it and the two progressive part methods. It might be noted in passing, nevertheless, that the mean scores of the nine subjects who employed the whole method are consistently inferior to the mean scores obtained with the two progressive part methods, while the percentages of retention are considerably greater. The apparent unpopularity of the whole method among the subjects studied is worthwhile bearing in mind when considering the possible effect of the practice factor in interpreting the results obtained in investigations in which the habitual learning methods of the subjects are unknown.

Besides the similarity of the methods, several other possible explanations for the absence of differences suggest themselves: our procedure of ques-

tioning the children did not separate the methods reliably; the children had not had sufficient practice in the use of their preferred methods to permit the appearance of a possible superiority of one method, the time allotted to learning was not sufficient to reveal a possible difference in efficiency between the two methods (6, p 191, cf also 1, p 729) The fact that no mean score was as large as 8 lines, or one-third of the poem, makes it possible that Sawdon's hypothesis concerning the relationship between the time allotted to learning and the relative efficiencies of the learning methods might be applicable From the data at hand, however, there is no way of evaluating these possible explanations, but the absence of a satisfactory explanation in no way calls in question the validity of the results obtained

In view of the assertion of the superiority of the whole method in recent texts in educational psychology referred to in the introduction of this paper, a brief review of the experimental data on memorizing poetry might not be unprofitable. With college students, Reed found no reliable differences in learning between the whole, progressive part, and part methods, while Stroud and Ridgeway found the whole method less economical in learning than the progressive part or part method, but found no significant differences between the three methods in retention With school children, Winch found the progressive part reliably superior to the whole in the learning of "short" poems, but obtained no valid difference between the two methods with a "long" poem In two experiments of the writer, there were no reliable differences between the whole and part methods in learning or retention, and in the present study there is no difference in learning or in retention between two spontaneously and habitually used forms of the progressive part method

It is clearly evident that the experimental data fail to support the alleged general superiority of the whole method Moreover, the data have not been sufficient, or systematic enough, to determine definitely the conditions upon which the relative efficiencies of the different methods of memorizing depend

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Stephens College
Columbia, Missouri

SOME SIMPLE OBSERVATIONS OF LEARNING

Goodwin Watson

A research chemist, a physiologist, a biologist, and a psychologist were sitting in the parlor after dinner on a rainy Sunday The other three felt that the weather did not invite them out to their laboratories, and that the psychologist should be able to find out something interesting whenever people were willing to serve as subjects The psychologist accepted the situation and quickly wrote out a series of lists of ten simple words each These were read to the three willing subjects under a variety of conditions Recall was tested after each presentation, the subjects correcting their own papers Two subjects were tested for recall after 24 hours. The instructions were rotated so as to avoid confusing any special ease or difficulty in a particular list with the advantages or disadvantages of a particular method

Comparison was made among the following procedures

1 Listen and try to recall

2 Listen, and as you think each word make a simple check mark on the paper. Make a general sort of check mark for each word[1]

3. Pay attention to the sixth, seventh, and eighth words in the list, because these are the ones that are usually overlooked

4 Try to get some attractive association with each word

5. Try to think of some very unpleasant association with each word

6 Try to combine the words into a unified Gestalt For example, the series, "wind, rock, dog, slave, ink, wheel, bread, beads, roof, star" might be remembered by envisioning a scene in which a strong wind blowing across the rocks ruffled the hair of the dog braced against it, while a slave down in the shelter below poured ink over a wheel, spoiling the bread, he did this because his attention was distracted by his interest in his beads, and he had been looking up over the roof to the star Much better scenes might be enacted with the same stimuli

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[1]This was suggested by Kurt Lewin's hypothesis (and experimental determination) that any sort of motor expression would, if repeated at the time of endeavor to recall, tend to improve the probability of recall, even though the same motor expression were used for all of the stimulus words

The numerical scores are the least interesting part of the observations made during this short experiment. They are shown in Table 1

TABLE 1
PERCENTAGE OF WORDS RECALLED

Method	Physiologist	Chemist	Biologist	Average
Immediate recall				
Listen (5 trials throughout the experiment)	55	94	79	76
Making a mark (3 trials)	58	92	67	72
Attention to center numbers (2 trials)	90	85	90	88
Attractive association (2 trials)	65	85	75	75
Unpleasant association (2 trials)	75	75	80	77
Gestalted (5 trials)	80	90	72	81
Recall of list after 24 hours				
Listen		34	14	
Mark		5	32	
Gestalt		77	13	
All words		44	11	
Words with which the subject was successful in immediate recall		43	13	
Words which the subject missed on original recall		50	8	

The most interesting part of such an experiment is the incidental observation of human behavior In this case, for example, the physiologist, being a lady somewhat advanced in years, a little less competent in the language of this foreign country than her fellow-subjects, and feeling presumably considerable need for status, felt the social pressure so strongly that she cheated rather regularly in reporting her scores. When the papers were later called for she was much embarrassed She had written in additional words which she counted at the time when they were read for correction The difference was marked in hand-writing between the words which she wrote during the authorized recall period and the words which she tried to add to the list later. The smuggled-in words were written smaller, in more irregular and definitely tense or restrained movements The same subject offered a series of excuses before beginning which might serve as alibis in case her record was not good, although the psychologist tried to make it clear that the only purpose was to compare an individual's performance under one set of conditions with his performance under another set of conditions, and did not involve any comparison among individuals Twice following the afternoon of the experiment, at periods separated by

several weeks, this subject stopped the psychologist to offer an explanation of the words that had been written in on the paper and the discrepancy between the words originally correct and the number reported. It was not hard to see in this very simple laboratory experiment that we were dealing with a lady who as a little girl had always gotten good marks in school, whose sense of values grew out of comparing herself with other people in a continuous competition, whose need to be well thought of was so great that the traditional objectivity of science could not prevail against it. This was the more interesting in that she was distinguished in her field and had been honored with research fellowships. I suspect that even the most ardent advocate of specificity in character would have trouble convincing himself after such a simple experiment that the research publications of this worker merited his entire confidence.

The errors made in reproducing the lists afforded another interesting possibility for psychologizing. That mental habit that Freud calls "condensation," and which appears occasionally in our slips of speech, was evident. The series of words, "rest, chicken, aunt, loss," produced a new word in its place, the word "lunch," which had not been mentioned. The series "noon, smiles, teacher" was omitted, but the word "school" was substituted. The series "summer, wood, play" was replaced by a new word "country." The type of error in which a related word was substituted was much more common in the attempt to remember by the use of some gestalted scene. Thus in that case "pig" was reproduced as "sow," "dirt" as "dirty," "roses" as "garden," and the like. Sometimes the imagined characteristics of a scene were more vivid than the stimulus words around which the scene was built. Thus in one list which contained the word "plate," it was followed by a new word, "broken," which was not directly suggested by any other word in the stimulus list. Such results in recall tend to support the idea that some kind of Gestalt is remembered, rather than a specific stimulus. It is a common observation that children asked to repeat the precise words of a sentence, as in the Binet tests, often repeat correctly the meaning of the sentence but interchange words, omit words, and substitute synonyms. When the emphasis was on meaning, as in constructing the scene, then the omissions and substitutes were of a sort that preserved the original meaning. When the emphasis was on simple memory of a nonsense series the substitutions were sometimes similar in meaning but were more apt to be similar in sound.

There was no evidence of learning, in the sense of improved ability to remember, with any method except that of the gestalted picture. The composite score, based upon all methods in each trial was 77% correct in the first trial, 82% in the second, 65% in the third, thereafter, 70%, 87%, 67%, 74%, 74%, 70%, 60%, 67%, and 77%. When the subjects tried to build all stimuli into one structured whole, the average was 60% in the first trial,

67% in the second, 77% in the third, 97% in the fourth, and 93% in the last The score for one subject was, for a list of ten words, 3, 5, 7, 10, 10 Why did learning take place only with this kind of method? Do our laws of learning lead us to expect that when type of stimulus, type of response, number of repetitions, knowledge of success or failure, and set in the sense of desire to learn are all constant, some ways of organizing reactions will make learning possible while others will not? Have we not omitted something important?

Introduction of any new method was frequently confusing One subject made a complete failure in the first trial with check marks. He remembered not one single word Similar observations have been made by Thorndike, but are they well explained in terms of connections between stimuli and responses? It seems clear that a change in "set" may disorganize the whole situation so that recall becomes impossible, even though recall was involved as a part of the "set" and recall was possible before and after this period of confusion One is inclined to believe that the naive explanation that this person was "confused," or "lost his head," comes closer to the truth than any explanation which relies largely upon a theory of separate and independent connections strengthened by use and satisfaction, weakened by disuse and annoyance. Not fading through disuse, but failure through disorganization, seems to be involved

The relative ease and difficulty of remembering any word depended noticeably upon its position in the series. This has long been familiar in the learning of lists of nonsense syllables Words falling in the sixth position showed only 47% correct, in fifth and seventh position, 67% correct; in fourth and ninth, 60%. The first and last words were 92% correct It is fairly easy to find laws of learning which attempt to describe this situation. the law of primacy for the first word, the law of recency for the last. That this explanation fits the facts seems doubtful Primacy, as Thorndike has shown, is by no means always an advantage in learning The extraordinary drop from 90% correct in the last word to 60% in the one immediately preceding looks suspicious If we had had nine words or eleven words, had paused for a time without speaking, we could very easily have demonstrated that it was neither length of list nor period elapsed since hearing which was responsible for the success with the tenth word It seemed to be the factor of lastness, a Gestalt quality closely related to the tendency to notice extreme figures in an array visually presented all at the same time I suspect that the experiments in which Thorndike failed to find primacy were series of unrelated elements with no gestalted unity.

When subjects were asked to concentrate attention on the middle terms of the series the effect was usually to improve the score Here again we have a law of learning which one would never be able to deduce from the

customary principles of exercise, effect, set, multiple response, etc. It amounts to a better distribution of attention over the learning field It seems to be a modification of the natural structure of the situation which overemphasizes firstness and lastness and obscures the middle. I dare say any examination of students' notes on lectures, plus their memory for them, would show the same structure generally prevailing Perhaps it takes special teaching techniques to make the middle of a lecture as clearly remembered, other things being equal, as the beginning and end The consequences of the instruction to pay attention to the middle numbers were equally interesting. The cases were few, but in no case was the last word forgotten Whatever learning energy was diverted toward the middle seemed to detract from ability to remember the first one or two words of the series. Thus what happened was something like a pretense that the series hadn't really begun until about the time when the fourth word came It was not the fact that physically these middle words were immersed in a number of other similar tasks, but the fact that psychologically they did not stand out, which had accounted for their previous dimness. *A change in the psychic organization, with no change in the physical stimuli, brought a different selective memory In traditional terms this is pitting the principle of "set" against the principles of primacy and recency, with a defeat of primacy but not of recency. In terms of structure, it is modifying the natural advantage of the end positions in any series by setting up what amounts to a new beginning point in the middle, but without really modifying the importance of the end-point.*

Another possible factor in determining which words would be remembered might be sought in their content Some words described family relations, others objects of the physical world, others, seasons, animals, etc In no case was there any evidence that words alike in meaning, or opposed in meaning, or belonging to the same general class, were any more alike in ease of recall than were words unrelated in meaning No more than one word of a type appeared in any one list Of course this failure of any class of words to stand out may well have been due to the conditions of the experiment All the words were common nouns of one or two syllables, selected to be as familiar as possible I venture to assert that a slight modification of the experiment could have shown that certain words—notably obscene words—would have been remembered regardless of their position in the series, the method of attempted recall, etc Is the extraordinary dynamic behind such learning adequately included in the laws of exercise and effect?

There was no evidence to support Lewin's interesting hypothesis that making some movement while listening would improve ability to remember

The attempt to connect some attractive experience with each word might be expected, according to a naive concept of the principle of effect, to

result in some increased tendency to recall the word Actually, of course, the satisfying operation, if any, is the associating rather than a recall operation Unless one can assume that the glow of satisfaction radiates over a considerable piece of the temporal environment, especially that which has preceded discovery of the satisfying associate, it is hard to see why the effect of the association should influence recall As a matter of fact, in this case it did not Activity in the direction of finding associations apparently reduced the effectiveness of remembering the list of stimulus words This may have been an implication for teaching Let us assume that a good teacher is interested in stirring up a mental activity Associations are sought by students It may be that these students will remember less well the series of stimuli given by the teacher than would be true in the case of students who did nothing but listen to the teacher. If two classes were tested for recall of a presentation, one class having made vigorous attempts to lead out from the presentation into some areas of mental activity, the other class having merely listened with intent to recall, the latter class may appear to better advantage Certainly this observation would suggest that attempt to use presentations as a starting-point for thinking does not increase ability to remember the presentation Apparently recall questions may not be used to evaluate teaching which is supposed to stir up activity of a different sort from the simple recall. It may be added that unpleasant associations were neither more nor less helpful than pleasant associations in the matter of recall of stimuli

The most successful method, once it had passed beyond the stage of novelty, was the construction of a unified Gestalt. One subject, a young woman of exceptional mental ability, had at least nine out of ten right in all trials after the first with this method Outstanding was the difference in her ability to recall words built into an imaginary scene and her ability to recall lists learned more mechanically. After 24 hours she remembered about one-third of the words to which she had listened passively, or to which she had responded by making little check marks, but she remembered 77% of the words which the day before had been built into the imaginary figures Moreover, in her case the words were recalled in the original order This was not true of words that had been remembered from the other lists As a rule, recall 24 hours later (the subjects did not know there was to be a recall) showed organization about the meaning of the term The subject remembered that the word "brother" had been in the list and quickly thereafter recalled "father" and "uncle" It was interesting that the memory for these words did not follow the "bonds" set up in the presentation although the recall situation was very much like that in which the words had been originally heard and recalled in the order of presentation. Why did they regroup themselves about categories of meaning? Is the best explanation that the previous bonds between words alike in mean-

ing had been often exercised? Hardly, if we conceive that exercise in any mechanical fashion. The sequence "brother-father-uncle" or "pig-dog-bear" is certainly rare The organizing principle in the brain must be of such *a nature that similarity in meaning is more potent than similarity in sound or contiguity in time and space Only in the case of the gestalted scene,* when the relationship of one object to the next in the series was meaningful, did the series order remain as stable. Mechanical sequence had to be worked over into meaningful interdependence, in order for the sequence to be recalled.

This article certainly does not aspire to contribute authoritative answers to questions concerning memory and recall It is based rather upon the assumption that the questions raised by a single observation may be quite as significant for scientific progress as the answers obtained by a mass experiment Here, of a rainy Sunday afternoon came evidence aplenty, in the analysis of the behavior of very few subjects in a very simple *situation, that many commonly accepted theories of learning and recall did not suffice. Let us review some of the questions*

1 How much of the background of the subjects and their character types must be taken into account? Clearly we were dealing here with a simple laboratory exercise, but even here a distorted personality reconstructed the situation to fit her emotional needs

2 Are we justified in believing that a personality pattern which showed itself in cheating on this simple exercise would reappear in many other situations in the life of this individual?

3. Why do words run together, "condense" into new ones alike in some sounds or meaning?

4. Why is it easier to reproduce meanings than to reproduce the stimuli, just heard, which set up the *meanings?*

5 What is the essential condition for learning (in addition to type of stimulus, type of response, desire to learn, number of repetitions, knowledge of success or failure, etc) which was present in the case of construction of a gestalted unit but absent in the other recall procedures which accounts for the fact that learning took place only with the gestalted imagery?

6 Why is shift to a slightly different method of trying to remember the list so upsetting in some cases but not in others? Are there types for which such shifts are relatively easy or hard? Again it must be remembered that type of stimulus, type of recall, desire to learn, number of repetitions, knowledge of success or failure, etc, were the same for trial "n," which showed large success and the next trial, "$n+1$," in which, with a slightly different method of listening, the subject *completely failed What nature of learning would correspond with such a striking possibility of disorganization?*

7. Should the law of primacy and finality be re-stated to emphasize the

condition that it is only when a series constitutes a sort of unit with beginning and end that these positions are especially favorable to recall? Is *it generally true both in perception and in recall that certain positions, in* relation to the structure of the whole, "stand out" while others are easily overlooked?

8. Why do some words (e g, those that refer to the private life of the subject) have such an extraordinary potency for recall?

9. Is there some explanation other than the fact of few cases upon which to base a judgment for the discrepancy between Lewin's observations and these here reported? If it be a law that accompanying motor expressions like check marks facilitate recall then it should appear in each case. Why did it not in these tests?

10. Under what conditions does a pleasant association with S strengthen the tendency to recall S? Here it seemed a handicap Is this related to the disorganization of the noticing and remembering process which has several times been suggested by our facts?

11 Why is the gestalted figure so much more vivid after 24 hours than the impressions made by the words reacted to in other ways? What is the meaning (in physiological reaction) of a "vivid" experience?

12 What principle of organization in the operation of the central nervous system would account for the fact that similarity in meaning is more potent than similarity in sound or contiguity in time or space?

13. Would American psychologists learn more important things more quickly if more attention were given to detailed analysis of a few cases in simple situations?

Teachers College
Columbia University
New York City

SHALL THE LEFT-HANDED CHILD BE TRANSFERRED?

C. E Lauterbach

In spite of the furor incident to the discovery of individual differences, the obvious answer to this question has not received the consideration it merits The positions taken by both psychologists and doctors of medicine (to whom the question is often put) are usually extreme—either that all left-handed children should be transferred, or that none should be transferred. Such an attitude ignores the fact that there are many degrees of handedness ranging from dominant left-handedness through ambidexterity to dominant right-handedness Whether or not a left-handed child shall be transferred depends upon the particular child and implies scientific diagnosis As will appear later, this diagnosis should involve four funda-

mental items: (1) the degree of handedness, (2) the type of personality, (3) motivation, and (4) method

A discussion of the question assumes transfer to be desirable. "Does the fact that you are left-handed ever inconvenience you or cause you embarrassment?" was put to 160 left-handed individuals with the following result:

Response	Frequency
No	77
Yes	61
Very little	3
Not yet	2
Too young to answer	8
No report	9
Total	160

Among those who said "No" quite a number had learned to perform certain acts with the right hand, a tacit admission of handicap The extent to which left-handedness is felt to be an embarrassment or an inconvenience probably depends upon the individual's vocation Many left-handers admit no handicap, and some feel quite "set up in the world" because of their extreme deviation from the average.

The situations in which embarrassment and inconvenience are experienced are too numerous to catalog here but those most frequently mentioned are

TABLE 1

SHOWING MEAN STANDARD INDEXES IN PENMANSHIP ARRANGED BY HANDEDNESS INDEX GROUPS

Group I represents those who have successfully transferred in penmanship from the left to the right hand; Group II, those who attempted transfer but failed, Group III, those who have always written with the left hand and have never attempted transfer, Group IV, those who have always written with the right hand. The Standard Index is found by multiplying Quality by Rate. The Standard Index for Grade VII is 1125 (12.5 x 90 on the Kansas City Scale)

HI*	Group IV f	Group IV SI†	Group I f	Group I SI	Group II f	Group II SI	Group III f	Group III SI	Total
0-19			40	616	91	674	90	537	221
20-39	1	240	55	738	38	673	25	700	119
40-59	4	760	43	731	12	576	9	552	68
60-79	39	765	14	819	3	867	2	601	58
80-99	459	860	11	752	2	750	2	868	474
Mean‡		854		714		669		578	

*Handedness Index.

†Standard Index

‡Found by multiplying Mean Rate by Mean Quality

eating and writing With reference to writing there is evidence that changed-hand writers (i.e., from left to right) are better penmen than unchanged writers Table 1 reveals such a tendency. Nine hundred forty specimens of penmanship were studied, arranged according to Handedness Index groups. Each specimen was scaled with the "Kansas City Scale for Measuring Handwriting," and the final quality score assigned to each specimen was the average of scores assigned by three different judges. In the table, rate and quality scores have been combined into a Standard Index found by multiplying the mean rate score of each group by the mean quality score of each group The mean Standard Index (the last row of the table) indicates that Group IV, the natural right-hand writers, with a mean Index of 854, are the best penmen The transfers, Group I, with an Index of 714, are next best. Group II, left-handers who attempted transfer but failed, with an Index of 669, rank third Group III, the left-handers who have never attempted transfer, with an Index of 578, rank lowest On the average the left-hand writers who have never attempted transfer are the poorest penmen. The changed-hand writers are the best penmen of the three left-handed groups.

The most striking fact brought to light by the data is that those left-handers who attempted transfer but failed write better than the left-handers who never attempted transfer It looks as if the intensive study necessitated by attempted transfer, even though unsuccessful, has resulted in considerable cross education The findings refute the popular criticism that the attempt to change a left-handed writer "spoils" his penmanship.

It should be noted that these general findings represent trends and that there are some exceptions Whether or not the data will hold for a school population remains to be demonstrated The mean age of the 940 cases is 20.8 years, which means that the group as a whole contained many adults The Standard Index for the seventh grade (according to the Kansas City Scale) is 12 5 x 90, or 1125 The highest Index achieved by any type group is 854. It is probable that this low Index is due to the number of adults in the population studied

A scale for measuring handedness and yielding a Handedness Index (HI) is described in (1) In Table 2 of this study HI's for a total of 1061 cases are shown, distributed according to writing habits The significance of the table, from the standpoint of diagnosis, lies in the percentage of successful transfers in the several HI groups (column 7) In the first HI group, 0-4, in a total of 29 attempts at transfer, none were successful In the HI group 5-9, in a total of 40 attempts, 14, or 35%, were successful In the next group the percentage rises to 50 The chances of successful transfer tend to increase with the HI

The data seem to indicate that a child with an HI of 0-4 cannot be successfully transferred In the case of a child with an HI of 5-9 the chances

TABLE 2

DISTRIBUTION OF THE HANDEDNESS INDEX GROUPED ACCORDING TO (I) SUCCESSFUL TRANSFER IN PENMANSHIP FROM LEFT TO RIGHT; (II) UNSUCCESSFUL ATTEMPT AT TRANSFER; (III) NO ATTEMPT AT TRANSFER; (IV) NATURAL RIGHT-HAND WRITERS

An Index of 0 represents complete left-handedness (as in the case of the amputation or paralysis of the right arm); an HI of 50 represents ambidexterity; an HI of 100 represents complete right-handedness

	Right-hand *writers*		Left-hand *writers*			Percentage *successful*
Index	IV	I	II	III	Total	transfer*
0-4			29	34	63	00.0
5-9		14	26	54	94	35.0
10-14		24	24	25	73	50.0
15-19		17	23	11	51	42.5
20-24		19	19	11	49	50.0
25-29		12	10	10	32	54.5
30-34		15	11	10	36	57.6
35-39	1	16	6	7	30	72.7
40-44	1	16	7	4	28	69.5
45-49		15	3	5	23	83.3
50-54	2	11	2	2	17	84.6
55-59	1	8	1		10	88.8
60-64	8	5		1	14	100.0
65-69	7	9	2	1	19	81.8
70-74	9	3	1		13	75.0
75-79	17	4		2	23	100.0
80-84	42	4		2	48	100.0
85-89	83	4		2	89	100.0
90-94	101	5			106	100.0
95-00	240	2	1		243	66.6
Total	512	203	165	181	1061	

*Groups I and II

are ten to three against successful transfer In the case of a child with an HI of 10-14 the chances are fifty-fifty In the case of HI's of 45 or over the chances are decidedly in favor of successful transfer

The degree of handedness, then, is an important factor in diagnosis Furthermore, it is one of the factors which can be measured with a reasonable degree of reliability

A study of the methods used in trying to bring about transfer sheds further light on this problem. Table 3 shows such data in the case of 356 individuals Under training were included the requirement of right-hand writing by the teacher, persistent changing of pencil and other tools (spoon,

TABLE 3
SHOWING THE FREQUENCY WITH WHICH VARIOUS METHODS WERE USED IN EFFECTING TRANSFER IN PENMANSHIP IN 356 CASES OF LEFT-HANDEDNESS

Method	I	II	Total	Percentage
1. Unconscious transfer	8	1	9	2
2. Training	109	110	219	62
3 Punishment	49	48	97	27
4 Rewards	3	3	6	2
5 Accident	4	2	6	2
6. Voluntary transfer	2	1	3	1
No report	14	2	16	4
Totals	189	167	356	100

TABLE 4
SHOWING THE DISTRIBUTION OF 37 CASES OF SPEECH DISORDER IN A TOTAL OF 1061 CASES OF HANDEDNESS GROUPED ACCORDING TO THE METHODS USED IN EFFECTING TRANSFER

	Unconscious transfer	Training	Punishment	Rewards	Accident	Voluntary transfer	Groups III-IV	No data	Totals
f	2	12	14				8	1	37
%	5	32	38				22	3	100

etc) to the right hand, persuasion, explanation, and encouragement. Such methods were frequently used and were often combined with outright punishment Undoubtedly some instances listed as training would be classed as punishment if all the details were known

The extent to which punishment was resorted to as a means of securing transfer is significant. Table 3 shows that in 27% of the cases studied punishment was used as a corrective Table 4 shows that among cases of speech disorder which involved transfer (columns 1, 2, and 3) punishment was used in 50% of the cases. The nature of the punishments employed is indicated by the following list

1 Tying gourd over left hand, wrapping cloth over left hand, compelling the wearing of a mitten
2. Tying left hand behind the back, tying fingers together
3. Cuffing, slapping, spanking, whipping, boxing ears, cracking the knuckles with a ruler.
4. Ridiculing, scolding, threatening.
5. Leaving the table, sending to bed, confining in closet
6. Keeping after school, giving low grades for work written with the left hand, refusing to accept work done with the left hand, doubling the amount of practice with the left hand

TABLE 5
SHOWING THE 37 CASES OF SPEECH DISORDER IN TABLE 4 DISTRIBUTED ACCORDING TO TYPE

Transfer group	Organic	Stuttering disappeared	Stutter now	Slight stoppage of speech	Nervousness	Lisping	St. Vitus dance	Totals
IV						3		3
I	3	7	5	2				17
II		2	7			1	2	12
III		2	1	1	1			5
Totals	3	11	13	3	1	4	2	37
Md HI	28	34	21	22	30	92	20	28

TABLE 6
DISTRIBUTION OF SPEECH DISORDERS IN RELATION TO HANDEDNESS INDEX AND TRANSFER IN A TOTAL POPULATION OF 1061 INDIVIDUALS

		Right-hand writers		Left-hand writers		
Index	Pop	IV	I	II	III	Total
0- 4	63			2		2
5- 9	94		2	2		4
10-14	73		1	1	2	4
15-19	51		1	2		3
20-24	49		1	2	1	4
25-29	32		2			2
30-34	36		2	1	1	4
35-39	30					0
40-44	28			1		1
45-49	23		3		1	4
50-54	17					0
55-59	10		1	1		2
60-64	14					0
65-69	19		2			2
70-74	13					0
75-79	23		1			1
80-84	48					0
85-89	89	1				1
90-94	106		1			1
95-00	243	2				2
Totals	1061	3	17	12	5	37

The nature of the speech disorders encountered in a total of 1061 cases is shown in Table 5 The median HI of this group is 28 In Table 6 all cases of speech disorder are grouped according to HI. Seventy-six per cent of the cases have HI's of less than 50; 46% have HI's of less than 25

These two tables seem to indicate that the majority of speech disorders are correlated with low HI's Just what the explanation for this fact may be does not appear from the data. An inference, however, is here ventured. Reputable authorities suggest that most cases of stuttering are caused by nervous instability. As indicated above, Table 4 shows that, in 50% of the speech-disorder cases involving transfer, punishment was used as a corrective The effect of these punishments cannot be appreciated by anyone who has never heard the wail of the unsuccessful transfer The conflicts that often take place are suggested by the following comment from a man sixty-five years of age Even after a lapse of three-score years resentment and emotion manifest themselves "My teachers made me miserable. I suffered much too much punishment and abuse from so-called teachers who could hardly write their names. The results nearly drove me from school." This subject's HI is 20. He reports no speech disorders But if, as may not infrequently happen, we impose such methods upon a timid, "nervous" child lacking in self-confidence and motive, we have an ideal situation for speech disorders *It is not the transfer per se which causes stuttering but the methods imposed upon a particular type of personality Any other finely coordinated activity forced by similar methods would bring about the same unfortunate result.*

The importance of motive in learning is too well understood to warrant discussion here. However, that this factor has been sufficiently stressed in accomplishing transfer is doubtful In the case of amputation or paralysis transfer is always effected and, so far as the author has been able to determine, without consequent speech disorders. The motive is compelling in such cases At Bluefield, W. Va., at the suggestion of the author, a group of high-school students formed a "Southpaw Club" to meet their extra-

TABLE 7

SHOWING THE END SCORES OF FOUR LEFT-HANDED HIGH-SCHOOL STUDENTS LEARNING TO WRITE WITH THE RIGHT HAND

Scores are in terms of Standard Index Norms are, for the 4th grade, 696, 5th, 799, 6th, 943, 7th, 1125

Students	End test	Score	End test	Score
A	First	405	79th	869
B	First	518	43rd	755
C	First	228	72nd	664
D	First	273	95th	970

curricular requirements The Club studied problems of handedness such as heredity, transfer, left-handedness in literature and art, etc. Four of these students undertook to learn to write with the right hand. Though habitual right-hand writing habits were not established, the results, shown in Table 7, *indicate what a well-defined motive can accomplish* The learning curve of Student D, covering 95 practice periods (more than three months), is shown in Figure 1 Furthermore, in dealing with left-handed children, it has been demonstrated to the author's satisfaction that bringing the left-handed pupils of a school together in a special group for penmanship instruction stimulates interest and competition and facilitates transfer.

The answer to the question proposed in the title to this article probably is that

1 If the Handedness Index is not too low, say 25 or better,

2 If the child is self-confident, bold, aggressive (not timid, fearful, nervous),

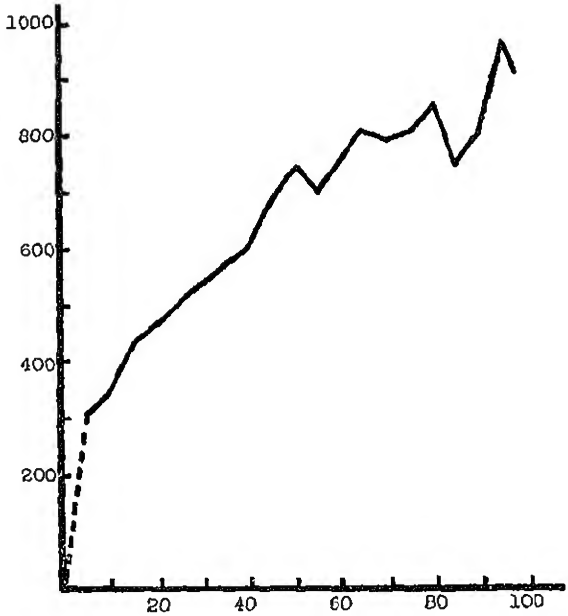

FIGURE 1

THE PRACTICE CURVE OF A LEFT-HANDED HIGH-SCHOOL GIRL LEARNING TO WRITE WITH THE RIGHT HAND

Her Handedness Index was 1; her age, 19 The horizontal axis represents scores in terms of Standard Index This girl's Standard Index for the left hand was 1197 The norm for the seventh grade is 1125

3. *If coercive methods are avoided and intelligent, informed supervision is provided;*

4. If a strong sustaining motive can be developed, transfer can be undertaken to the advantage of the child with a reasonable expectation of success If one or more of these factors are lacking, transfer should be undertaken with caution

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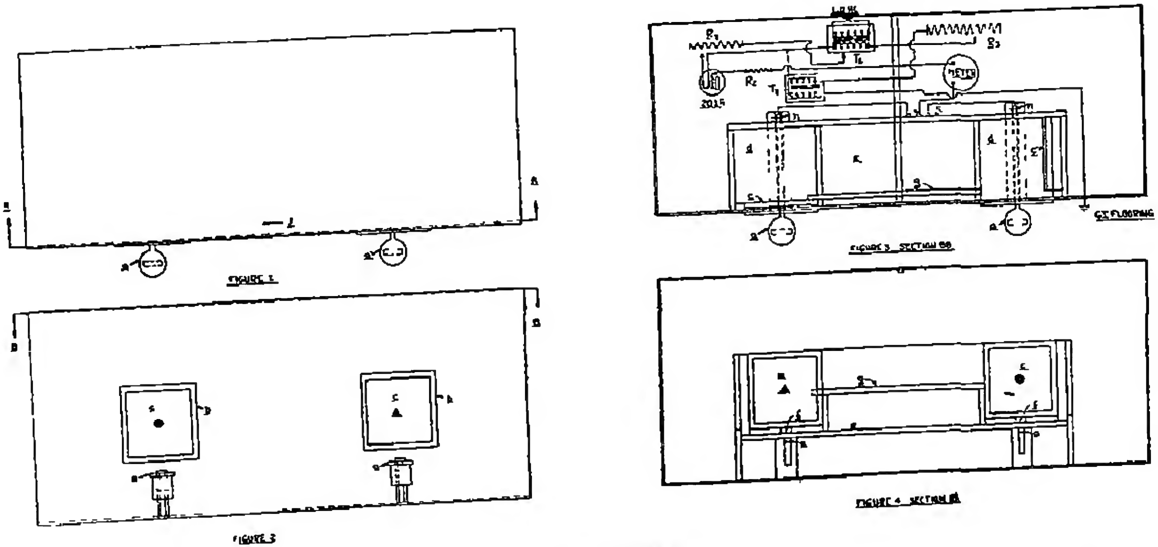
West Virginia Wesleyan College
Buckhannon, West Virginia

APPARATUS AND TECHNIQUE FOR THE STUDY OF DISCRIMINATION IN THE HIGHER VERTEBRATES

K U. SMITH

In connection with a series of experiments on form discrimination in the cat, the apparatus shown in the accompanying diagram has been found to be well suited for the study of visually determined responses of this animal The apparatus consists of a box 48 ins long, 18 ins wide, and 20 ins. high, painted flat black. Two doors (*c* and *c'*), mounted in the front panel, swing inward by means of springs attached to their rear sides Their front sides are fitted with grooves into which cards bearing various types of visual stimuli may be easily inserted or removed Inside the box, directly below the doors, is a shelf (*e*) for food containers. This shelf is so constructed that the doors may swing inward without coming into contact with the food containers placed immediately behind them. At the back of the shelf and to the right and left of the doors are partitions which form the enclosures (*d* and *d'*), into which the animal thrusts its head. The doors are held tightly closed by spring latches (*f* and *f'*), to which are attached small brass levers (*a* and *a'*) that extend 4 ins. outward from the box and terminate in circular brass plates of a size ample to accommodate the paw of the animal The cat obtains the food placed in the containers by pressing down one of the levers and then thrusting its head through the aperture of the opened door Either door may be locked by the sliding master-latch (*g*), and the animal may be punished for pressing either lever by the shocking circuit at the rear of the apparatus

During experimentation the animal is alone in the room with the apparatus, observations and motion pictures of its behavior being made from an adjoining room through a small opening in the door The box is placed along one of the walls of the room and is illuminated by flood lights from the opposite wall The cat is first trained to obtain food merely by pressing the levers and thus opening the doors. Training with visual stimuli is then



Plan of the Apparatus

Except for the shocking circuit the plan of the apparatus has been drawn to a 1-to-4 scale, and represents a top and front view together with two corresponding cross-sections *a* and *a'*, levers, *b* and *b'*, apertures, *c* and *c'*, doors; *d* and *d'*, enclosures in which food is placed, *e*, shelf on which food-containers are placed, *f* and *f'*, small spring latches, *g*, sliding master-latch, *h* and *h'*, pinions for the levers, *i*, handle of the top of the box The resistances in the shocking device, *R1*, *R2*, and *R3*, are 20, 250,000, and 12 ohms respectively *T1* is a 75-watt toy transformer, *T2* a T-model Ford spark-coil

begun. In the course of this training the animal learns, when released from a restraining cage placed 8 ft. in front of the apparatus, to approach and press the levers with reference to the stimuli displayed on the doors. If, in any trial, the lever pressed is "correct" for that trial, the door opens and the animal thrusts its head inside the box and obtains the food. If the lever pressed is "incorrect" for that particular trial, the animal receives a slight shock from the circular brass plate. After it has obtained the food either by going directly to the correct door, or by getting a shock and then going to the correct door, it is returned to the restraining cage and the procedure repeated. Alternations in the position of the positive or "correct" stimulus are made constantly and irregularly. Food is placed behind both doors.

The apparatus is constructed of ½-in. soft pine, except where otherwise indicated, and has a hinged top which may be made to remain open by means of a sliding catch. The front panel is made of ¼-in. pressed wood. The two apertures (*b* and *b'*) are 6½ ins. square and are framed with pine strapping, 1/8 in. thick and 1/2 in. wide. Their centers are 23½ ins. apart, and their lower edges are raised 6½ ins. from the floor. Below these larger apertures, and framed in the same manner, are two smaller openings ½ in. wide and 4 ins. high. Extending through these smaller openings from the inside of the box are two levers (*a* and *a'*). They consist of ¼-in. square brass rods, 12½ ins. long, to the outer ends of which are screwed 2½-in. circular brass plates, fitted on their under sides with small pieces of wood 2 ins. by 2 ins. by ½ in. The pinions (*h* and *h'*) serve as fulcrums to the levers, which are held to the top of the smaller apertures by springs inside the box.

The doors (*c* and *c'*) which cover the larger openings are 7½ ins. square and fitted along their edges with grooves made by overlaying 1/8-in.-by-1/2-in. strips of wood. These grooves open at the top for convenience in inserting the stimulus cards, which are 6½ ins. square.

The shelf (*e*) which is 7½ ins. wide and 35 ins. long, extends across the inside of the box 2 ins. below the lower edge of the larger openings, and 4 ins. above the floor of the inside of the box. It is mounted between two end-pieces, 7½ ins. wide and 6½ ins. high, and rests upon two other pieces of wood, hard pine, as shown in Figure 3. Rectangular sections, ½ in. by 3½ ins., are removed from the hard pine pieces to accommodate the levers, which are arranged as shown in Figure 4. A board of the same length as the shelf, and 8½ ins. wide, is mounted along its rear edge, while two boards, 7½ ins. by 5½ ins., are mounted on the shelf at the center side of each of the doors. Rubber stops are so placed on the end-sections of the shelf that neither of the doors can swing through an angle of more than 45 degrees. Thus, when either door is open, as shown in Figure 3, one of the enclosures, *d* or *d'*, is formed, into which the animal may thrust only

its head and forebody, the interior parts of the apparatus remaining always hidden from the animal's view

Immediately below the doors on the inside of the front panel are the spring latches (*f* and *f'*), their catches being screwed to the lower edges of the doors The blunt ends of the latch plungers are drilled and fitted with small hooks, as are the lever rods at a point just inside the front panel Small springs of slightly greater tension than that of the latches are attached to these hooks. Thus the door can be opened at the first touch of the lever, though the levers can be pressed down to the floor. The master-latch (*g*), which determines which door the animal may open, is a sliding strip of oak ½ in. by 18 ins, mounted in grooves in the cross pieces of the shelf, as shown in Figures 3 and 4

A shocking device described by Howells (1) has been modified to suit this apparatus (*T2*) is a Ford spark-coil with the interrupter crossed. The secondary lead-off from the transformer (*T1*) is 5½ volts. The electrodes of the shocking circuit are the brass levers, *a* and *a'*, and a sheet of galvanized iron, 2 ft by 4 ft, on which the cat stands, in front of the box.

With this apparatus and technique, and using a very slight electric shock introduced between the 150th and 200th trials, discriminatory responses have been developed in nine cats. Two animals learned to discriminate a 4-in. white circle from a white triangle of equal area, both exhibited on black cards, with 70 and 78% of correct responses respectively in the last 25 of 200 trials The experiment was then temporarily discontinued because of the termination of the school year. Three additional animals have since been trained to visual stimuli displayed in the doors of the apparatus. Two of these animals were trained to discriminate a 4-in black circle from a black triangle of equal area, both exhibited on white cards, and required 300 and 900 trials respectively to reach 90 to 100% correct choices of the triangle in a set of 25 trials The third animal, trained to respond to a 4-in white circle and a white triangle of equal area both exhibited on black cards, required 400 trials to reach 90 to 100% correct choices of the triangle in a set of 25 trials Check experiments run on these three animals showed the responses to be determined by the stimuli displayed in the doors of the apparatus Four additional animals have recently been trained to respond to a 4-in white circle and a triangle of equal area displayed in the doors These animals were not punished by electric shock for an "incorrect" response to the circle. From 90 to 100% correct discrimination of the triangle in a set of 100 trials has been established in these animals within 500 trials Check experiments are now being carried on in the case of these four animals All cats so far employed in the experiment have readily learned to manipulate the apparatus and to show the presence of visual responses to the stimuli displayed in the doors.

The results obtained indicate that this apparatus has the advantage, not only of eliciting a definite response which can be demonstrated to be a function of the visual stimuli presented, but also of providing general experimental conditions that are easily controlled Olfactory influences, for example, are controlled by placing food behind both doors, by alternating the position of the stimulus cards independently of other parts of the apparatus, and by exchanging used for fresh cards during experimentation. Auditory cues and extraneous cues from the experimenter are excluded since the cat itself manipulates all movable parts of the apparatus when making its discrimination, and since the animal is alone in the room from the moment of its release to the moment of obtaining food The only sliding door used, that which releases the animal from the restraining cage, is controlled from outside the room.

This apparatus, it is believed, can be readily adapted to studies in discrimination of brightness or of auditory stimuli in a variety of the higher vertebrates, particularly those dextrous in the use of their forepaws

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Brown University
Providence, Rhode Island

FOURTH- AND FIFTH-GRADE STANDARDS FOR PHOTOGRAPHIC EYE-MOVEMENT RECORDS[1]

Alvin C Eurich

The usefulness of photographic eye-movement records for elementary-school pupils would be enhanced considerably if adequate norms for the various grade groups were available From the records obtained at the present time it is difficult to state whether the measures for a particular child are equal to the average, the poorest score, or the highest score for his grade Furthermore, when records are secured at several institutions, not only the reading matter but the style and size of type together with the length of lines differ enormously As a result it is impossible to accumulate records and build up a set of standards comparable to those ordinarily

[1]The writer is indebted to the Graduate School of the University of Minnesota for financial aid to conduct this study, to Alice Albert Eurich for photographing the eye-movements of the entire group, and to Mr H C Cooper, Principal of the Tuttle School, for his constant willingness to cooperate in studies of this type

arranged for the better achievement tests. The results reported in this paper represent one attempt to provide more adequate standards for photographic eye-movement records of elementary-school children

Throughout the spring of 1932, pupils in the 4A, 5B, and 5A grades at the Tuttle Elementary School in Minneapolis were transported in groups of from four to seven to the University of Minnesota campus where their eye-movements were photographed while they read the following three paragraphs

I He had in his house a housekeeper past forty, a niece under twenty, and a lad for the field and market place, who used to saddle the hack as well as handle the pruning fork. The age of this gentleman of ours was bordering on fifty; he was of a hardy habit, spare, gaunt featured, a very early riser and a great sportsman

II Ants live in hills or nests made of sand or dirt Many ants work together to make one nest or ant city One ant hill has often as many as two hundred houses A nest is sometimes a tunnel in the earth and sometimes a large hill with hallways under the ground

III They went across the hall to a door at the back of the house. It opened before them and disclosed a long, bare, melancholy room, made barer still by lines of desks At one of these a lonely boy was reading near a feeble fire; and Scrooge sat down upon a form, and wept to see his poor forgotten self as he had used to be

Paragraphs I and III were printed in twelve-point Caslon type with 80-mm lines Fourteen-point Caslon type and 100-mm. lines were used for Paragraph II Immediately after reading each paragraph the child was asked four questions to determine whether he understood the content of the reading matter

Arrangements were also made to check the effect of practice in reading paragraphs under the conditions necessary for photographing eye-movements while reading Approximately one-half of the pupils read the paragraphs in 1-2-3 order while the remaining pupils read the set in 3-2-1 order. It was possible, then, to compare the records of the children who read Paragraph I first in the series with the records of those who read the same paragraph last. In other words, a comparison was obtained between the records of pupils without previous practice and records secured after the two paragraphs had been read While the amount of practice is relatively small, it nevertheless affords an opportunity for the child to become adjusted to the strange conditions and for his eye to become adapted to the beam of light reflected from the mirror The same type of comparison was possible for Paragraph III

To make certain that the group reading a particular paragraph first was somewhat similar in ability to the group reading the same paragraph after practice the average intelligence quotients on the Pressey Intermediate

TABLE 1
AVERAGE INTELLIGENCE QUOTIENTS ON THE PRESSEY INTERMEDIATE CLASSIFICATION TEST FOR PUPILS WHO READ PARAGRAPHS I AND III FIRST AND LAST IN A SERIES OF THREE

Grade	N	First Reading M	SD	N	Last Reading M	S.D
			Paragraph I			
5A	16	118.4	12 3	16	115 1	10.3
5B	14	107 7	12 9	10	110 5	14.3
4A	13	119 2	9 9	9	111 5	14 8
Total	44	115 3	12 7	36	113 0	12.7
			Paragraph III			
5A	18	116 0	9 8	12	119.2	13 9
5B	11	112 2	14 4	10	109.1	11 5
4A	11	115.0	14.6	11	117 7	10 8
Total	40	114 7	12 7	33	115 6	12 9

Classification Test were compared (Table 1) It is clear that, as groups, the children are consistently above the average of a large unselected population since the average IQ's vary from approximately 109 to 119 In none of the half-grades represented or in the total group may the difference in

TABLE 2
DIFFERENCES BETWEEN PHOTOGRAPHIC EYE-MOVEMENT RECORDS ON PARAGRAPH I BEFORE AND AFTER PRACTICE

Grade	First reading Mean	Last reading Mean
	Number of fixations	
5A	73.25	65.61
5B	73.40	74 45
4A	97 38	90 66
Total	82.68	74 48
	Number of regressions	
5A	15 06	12 88
5B	14 75	14 81
4A	24 84	23 55
Total	18 21	16 60
	Total perception time	
5A	1049 50	894 66
5B	1115 70	1126 18
4A	1469.15	1264 88
Total	1243.78	1043.58
	Average duration of fixation	
5A	14 47	13.60
5B	14.93	14 66
4A	14 95	13.72
Total	14 79	13 74

ability between the pupils who read Paragraph I and those who read Paragraph III first be designated as significant. Not only can it be assumed, but it has been demonstrated, that the groups are equal in intelligence as measured by the Pressey test.

Throughout Table 2 are presented the data showing the differences in eye-movement records between the groups reading Paragraphs I before and after practice. As given in the table, the number of fixations includes all fixations regardless of whether they occur after a forward or regressive movement. The total perception time has been determined from the number of dots on the record for Paragraph I, each dot representing 1/50 of a second. In view of this fact the norms for perception time are given in fiftieths of a second. The average duration of fixation has been determined for each pupil by dividing the total perception time by the number of fixations. While merely the averages for these four measures are shown in the table, a statistical evaluation of the differences between the two groups revealed none that was significant. A consistent trend is evident, however, for the pupils with practice to average fewer fixations, fewer regressions, a lower total perception time, and a lower average duration of fixations.

TABLE 3

DIFFERENCES BETWEEN PHOTOGRAPHIC EYE-MOVEMENT RECORDS ON PARAGRAPH III BEFORE AND AFTER PRACTICE

Grade	First reading Mean	Last reading Mean
	Number of fixations	
5A	75.22	75.60
5B	77.14	68.20
4A	98.72	91.81
Total	80.48	78.34
	Number of regressions	
5A	16.77	17.46
5B	17.14	12.70
4A	22.00	22.81
Total	18.53	18.29
	Total perception time	
5A	972.22	1022.60
5B	1096.42	980.30
4A	1371.27	1309.36
Total	1013.35	1083.55
	Average duration of fixation	
5A	12.89	13.52
5B	13.91	14.10
4A	13.12	13.99
Total	13.42	13.88

The figures in Table 3 for Paragraph III are similar in showing no significant differences between the groups although the trend is not as

consistent as for Paragraph I For example, the 5A group reading the paragraph after practice obtained slightly higher averages for fixations, *regressions, and perception time The net result leads to the generalization* that the practice in reading two paragraphs does not affect the average records for the groups significantly. Whether more extensive practice would alter this deduction is a query that awaits further study

Careful scrutiny of Tables 2 and 3 will reveal that, in general, the records improve definitely from Grade 4A through Grade 5A In time, therefore, as the number of cases accumulates, it will be advisable to establish separate norms for each half-grade For the present the averages in these tables will serve as tentative standards for the separate grade groups, while the figures in Table 4, summarizing the records of all children who read

TABLE 4
FOURTH- AND FIFTH-GRADE NORMS OF EYE-MOVEMENT RECORDS ON PARAGRAPHS I, II, AND III

	Fixations	Regressions	Total perception time	Average duration fixation
		Paragraph I (N=78)		
Q_3	93 06	23 74	1352 33	16 03
Md	76 07	16 06	1065 02	13.88
Q_1	62 65	10 31	825 50	12 72
		Paragraph II (N=85)		
Q_3	77 90	23.19	1078 84	13 99
Md	67.89	15 66	855.04	12 64
Q_1	56 66	9 69	690 62	11 51
		Paragraph III (N=73)		
Q_3	92 75	25 32	1287 98	14 93
Md	71 78	16 26	959 72	13 50
Q_1	61 02	9 89	748.80	12 07

the three paragraphs regardless of order, will provide more adequate norms for the combined grades Within this table, Q_3, or the 75th percentile, the median, and Q_1, or the 25th percentile, are given so that one can readily determine whether a pupil is among those with the best or the poorest records. An illustration may clarify the use that can be made of these standards Presume that Peter, who is in the sixth grade, read Paragraph I with the following record total fixations, 102, regressions, 25, total perception time, 1250/50 of a second, and average duration of fixations, 12/50 of a second It is immediately clear that this child falls within the highest fourth of the combined-grades group in number of fixations and regressions A very inferior record is revealed thereby since presumably the greater the number of fixations and regressions, the less efficient is the reader In total perception time Peter is above the median but does not reach Q_3, while in average duration of fixations he is slightly below Q_1

An interpretation of this record would warrant the generalization that Peter's perception of printed words occurs quickly whereas his perception span is narrow The data for Paragraphs II and III may be used similarly

Information concerning the reliability and validity of the records has been published elsewhere (1, 2) In general, it reveals that the measures of eye-movements are usually reliable, since for single paragraphs, which on the average take less than one minute to read, the coefficients of reliability for fixations and regressions vary between 70 and .87, while for the three paragraphs combined the reliability coefficient is above 90 For the average duration of fixations, the reliability is somewhat lower but still adequate for group measurement

The validity of the records as measures of reading ability remains uncertain For more than thirty years it has been assumed generally that measures of eye-movements are the most valid indices of a child's ability to read In fact, this assumption has been accepted so completely that practically no attention has been directed to an evaluation of the records When evaluated recently in terms of standardized reading examinations (both rate and comprehension), achievement in school, and intelligence test records, the relationships were found to be surprisingly low. It appears quite plausible that the conditions under which the eye-movements are photographed are so unlike the normal reading situations that the records are not measures of ability to read except under the peculiar conditions provided *However, the records improve very noticeably from one half grade* to the next and may, therefore, be diagnostic of definite maturation or lack of growth in some significant trait. If further studies prove that photographic eye-movement records have value in diagnosing a pupil's difficulties even though the relationship to stock reading tests is low, then the writer plans to establish norms throughout as wide a range of grades as possible. Until then such records must be used cautiously even with the tentative norms reported herein

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University of Minnesota
Minneapolis, Minnesota

A STUDY OF FRIENDSHIPS AMONG ADOLESCENT BOYS

E. DeAlton Partridge

Early attempts by psychologists to explain group formations and activities were in terms of the gregarious instinct. According to this theory, humans, like many of the other higher animals, have an inherited disposition to prefer association with other members of their own kind. This instinct, it is claimed, finds expression in the group activities common to man the world over. Societies, tribes, secret orders, etc., are all the result of man's desire to be with his fellows, thereby satisfying this inborn urge. Volumes have been written outlining the rôle of instincts in human relationships. Unfortunately, this hypothesis left the problem at rest for some time and little progress was made in the actual observation of groups and the manner in which they operate.

Still another attempt at explanation of these collective human phenomena are the sociological laws of Thomas and Znaniecki (7, pp. 72-73). These authors catalogue human social activity into various wishes. Among those listed are the well-known four as follows: the desire for new experience, for recognition, for mastery, and the desire for security. Like the instincts, these "laws" do not explain for us *how* individuals choose their associates. An earlier writer, Giddings (5, pp. 17 ff.) ascribes this tendency to "consciousness of kind" and suggests that this acts upon human society to create cultural, political, and racial groups. The latter-mentioned theory seems to be nearer the truth. It may or may not be true that human society is founded on basic urges or instincts of the organism—but we should know in addition to this why certain people prefer the association of *certain other* individuals. What is back of the selection of friends and associates?

It is comparatively recent that experimental psychologists, not satisfied with the explanation offered by the instinct hypothesis or social laws, have been making studies on the nature of the group and the individual as a member of it. Most of these studies have centered around children of school or preschool age. Space will not permit a survey of all these studies at this time, but it is quite important that we outline the implications they have made. We shall not be concerned about proving or disproving the instinct hypothesis. We are concerned about the underlying causes of *certain* group formations. It seems hardly necessary to justify the assumption that human beings *do* settle into small groups. Thrasher (8) in his study of gangs in Chicago found no less than 1313 different groups, most of them composed of adolescent boys. These groups were definite enough in most cases to have names and recognized leaders. Each of these groups represented a source of common knowledge, common attitudes, and common reactions. What were the underlying causes of the members of these groups seeking each others companionship?

Buhler (2) has assembled many interesting and significant studies of social behavior in children She presents evidence that social behavior manifests itself as early as three months Results from German, Austrian, Russian, and American observers show that there is a tendency for children of all ages to move in groups of various sizes—the size of the group and the definiteness of its organization dependent upon the ages of its members.

Furfey (4), attempting to ascertain the reason for friendships among boys, set about to measure seven different factors in the choice of companions These factors included grade location, neighborhood, chronological age, mental age, developmental age (or social maturity), height, and weight One interesting fact brought out was that boys chose, nearly always, other boys from their own neighborhood or class at school. In fact, only one pair of boys of the 62 pairs studied were not of the same neighborhood or class at school While he found correlations ranging from 22 to 39 among the individual characteristics, he concluded that the essential condition for the formation of friendships is the classroom or the neighborhood Within the same neighborhood, however, the boys seemed to choose others of the same age, size, intelligence, and maturity.

Another study by Warner (9) of the influence of mental level in the determining of boys' gangs showed that the boys differed much more in chronological than they did in mental age The mean variation of chronological age was twice as great as that of mental age While it was found true that proximity of homes, common experiences (such as selling papers), and other factors did bring boys together, it was really the mental ages that kept them together It was quite evident from this study that social standards, experience, and education do not affect boys as much as adults in the choice of their companions.

Williams (11) reports that, according to the reasons stated by 84 boys, they choose friends because they "are full of fun," "fair and square," "friendly," "have done things for them," "are good sports," "athletes," "stick to you," "live near," "are quiet," "good boys," "help in trouble," etc. His study also brings out the tendency to select boys of the same chronological and mental age. While it is questionable whether we can rely on the boy's own interpretation of why he selects other boys for his friends, it is interesting to note how many of the factors mentioned have been corroborated in research

Similar studies by Almack (1) and Detroit Teachers College (3) tend to confirm the previously mentioned investigations with varying stress on the importance of intelligence as a factor A more objective study has been reported by Wellman (10) who observed closely the children who were seen together over a period of five months at Lincoln School, Teachers College, Columbia University Twenty-seven pairs of girls and 29 pairs of boys were arrived at in this manner and compared in regard to chronological

age, mental age, intelligence quotient, scholarship, extroversion, height, and physical achievement. *The girls were found to be more alike in scholarship* and less alike in height, while boys picked friends of their own height and intelligence quotient, but less alike in mental age, extroversion, and scholarship

Hartshorne and May (6) found in their studies of character that pupils conformed more nearly to the standard of their own circle of friends than to that of any other group, that is, than to their home background, their parents, or their teachers Either children of like conduct associate together because of the fact that they are alike and therefore congenial or they become alike by association The other studies outlined would indicate that the former is true, namely, that children of like moral standards find a common interest in each other

The present study is one made in a summer camp for boys ranging in age from 12 to 17 years The summer camp offers an unusual opportunity to study the nature of social groups. The campers are in relatively close contact with each other for many days They have ample opportunity to observe each other in many different situations and, as a result, the choices of friends are made culminating many different common experiences The matter of chance acquaintance is largely eliminated in such an environment especially if the boys are in camp together for several weeks In the present case the minimum period of attendance was for two weeks Seventy-two of the 142 boys were in camp for a month or more

The first two days in camp the boys were told to shift about and to find the group they wished to be associated with while in camp They were told that the entire camp would be divided up into groups of eight boys These groups were to select their own leaders from their number and were to eat together, sit at the council fire together, and in various ways be made conscious that the group was an administrative unit At the end of two days the permanent groups were formed and the boys remained in the group of their choice for the rest of the week, when they were allowed to change if they desired This procedure was followed throughout the summer and careful record was made of the composition of the groups It can be seen that during his stay in camp each boy had ample opportunity to shift about until he found a congenial crowd

During the camping season each boy was given the Army Alpha Group Intelligence Test An analysis of the scores made by the campers shows a distinct tendency for the boys to shift to their own mental level in selecting a group (or in the group selecting him as was often the case). As will be seen from the accompanying table, the average score for the group on Army Alpha was 112; the S D was found to be 31 4 for the entire group, indicating a wide variation in scores. The range was unusually wide, being from 34 to 190

TABLE 1

Analysis of Group Formations in a Summer Camp for Boys, Giving Ages in Months, Score on Army Alpha, and Size of Groups
Total Boys in Study 142

Group	Size	Army Alpha Mean	Army Alpha S.D.	Age in months Mean	Age in months S.D.
Entire	142†	112.2	31.4	168.1	15.5
1	8	129.1	37.1*	171.8	17.4*
2	7	100.1	28.6	155.6	15.9*
3	9	109.9	25.2	162.2	15.2
4	7	112.0	12.8	162.7	10.8
5	8	118.5	16.5	176.4	19.5*
6	9	108.6	30.9	164.0	11.0
7	6	112.2	21.6	177.8	16.3*
8	8	115.8	18.4	162.3	7.9
9	7	123.7	31.6*	166.3	16.8*
10	8	105.8	37.9*	172.3	6.6
11	8	95.8	34.7*	172.9	7.8
12	8	108.0	15.3	169.1	15.0
13	8	110.6	25.4	156.5	8.6
14	8	117.0	18.1	158.8	13.0
15	8	146.0	20.8	179.1	10.3
16	8	110.6	26.5	174.6	13.9
17	9	112.9	21.4	162.9	7.8
18	8	113.3	30.8	158.5	13.0
19	6	117.7	16.1	177.3	16.6*
20	6	111.7	18.0	156.0	8.2
21	7	113.5	19.8	181.4	8.2
22	6	115.6	19.9	166.8	17.8*
23	8	89.5	41.4*	172.8	11.3
24	4	119.8	19.7	160.2	9.0
25	6	101.7	28.6	164.7	11.9
26	7	133.5	19.9	162.5	10.1
27	7	120.9	15.9	172.7	8.6
28	7	112.9	27.9	176.3	15.4
29	6	124.7	27.9	166.2	12.6
30	4	88.2	24.1	177.0	29.8*
31	5	100.4	40.6*	175.4	18.3*
32	6	104.0	29.0	163.4	13.9
33	7	125.4	25.7	164.0	12.0
34	6	124.4	19.3	171.0	7.5
35	4	112.8	20.3	178.2	17.6*
36	4	136.5	27.4	170.8	11.9

*Exceeds S.D. for the entire group

†The total of the groups does not equal this because of changes in the group personnel. Some boys were members of two groups.

It will be seen that the S.D.'s for the small groups range from 12.8 to 41.4 with *only six of them equal to or exceeding* the S.D. for the entire group. At least two of the groups which exceeded the S.D. for the entire group can be accounted for by the fact that the boys were from a different town and, being in camp for only one period, decided to try their plight together. There is little doubt that if these boys had been longer in camp this grouping would have given way to the selective influence of mental level.

In order to compare these findings with the effect of age upon the formation of the groups, a similar analysis of the age groups was made. The mean age for the entire group was found to be 168 months, the S.D. 15.5. From the table it will be seen that the smaller groups ranged from 7.5 to 29.8 in S.D. and that *10* of them had S.D.'s above that of the group as a whole. Evidently, even as rough a measurement as Army Alpha is a better means of predicting which boys will associate together than is age. A survey of the means for the smaller groups indicates the wide variation there, and is another indication of the tendency for like to seek like mentally.

As a further check on the tendency for boys to select other boys of their own mental level, a tabulation was made of the boys' best friends during camp as reported by them at the close of camp. Each boy was asked to list three other boys he "chummed" with in camp. A check was then made to see which of the boys were "mutual" in their selection of friends, that is, which of them named each other. It was thought that this would help eliminate the factor of popularity. Computing the Pearson product-moment correlation for these choices it was found to be 4285 for score on Army Alpha. In the matter of age the correlation between that of a boy and the friend he listed was found to be *3721*. While this is not a statistically reliable difference it is certainly suggestive in light of the foregoing results.

The results of these studies have a direct bearing upon educational methods in respect to grouping of children. There is little doubt that if pupils were allowed to arrange themselves into their own groupings there would be a selective factor of intelligence involved, that is, they would tend to select children of their own mental level. Conversely, one would expect that when children were sectioned according to mental age they would tend to be better adjusted to the school situation than would be the case if they were expected to make an adjustment in a heterogeneous group *in respect to mental ability.*

Summary

1. Many studies have shown the tendency for children to select other children of like mental, social, and moral development for their friends.

2. The present study shows that when adolescent boys are permitted to select their own groups in summer camp there is a distinct tendency for them to (*a*) select boys of their own chronological age and (*b*) to select

from their own chronological ages those boys of a like mental age

3 Boys reporting other boys as their best friends during the summer tend to list boys who are nearer to them in mental age than in chronological age

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Boy Scouts of America
2 Park Avenue
New York City

BOOKS

NORMAN L. MUNN *An Introduction to Animal Psychology The Behavior of the Rat* Boston Houghton Mifflin, 1933. Pp xxii+439

Munn has written a text which is a worthy successor to Watson's classic *Behavior An Introduction to Comparative Psychology* of 1914 The accuracy and thoroughness of the book, its very clear and economical style, the profusion of well-chosen illustrations, and the extensive bibliographies combine to make this book a well-nigh indispensable tool for research workers in this field and an unusually satisfactory text

As the subtitle of the book indicates, the discussion is restricted, except for a very few incidental references, to studies of the behavior of the rat This limitation of the book is, however, one of its sources of strength, whether the book be viewed from the research worker's standpoint or from the standpoint of a teacher The book is coherent and free from confusion to a degree which could hardly have been attained if the whole range of animal psychology had been covered Psychological literature on the rat is, of course, extensive, and only the restriction of this book to this one animal has permitted the presentation of this literature in a usable, rather than in a compendium, manner. Furthermore, as this book proves, the literature on the rat has been quite well-rounded (except, perhaps, with regard to field studies of the rat in wild life, which might prove, judging from anecdotal material, a rather fertile field and a rather suggestive background for experimental studies) Of course, rat psychology is not comparative psychology, but the advantages of this book are so marked that it seems fortunate that the author chose the plan he did. It would be most desirable for some psychologist (either Munn or someone else who will work with equal care) to provide a companion volume on "The Behavior of Animals Other Than the Rat", but whether or not this is done, the present volume will probably remain one of the important psychological texts for some time to come

A brief sketch of the contents will indicate, in part, how comprehensive the book is The first of the seven chapters deals with unlearned behavior. The four divisions of this chapter deal with fetal behavior, non-orienting responses of young and adult animals,[1] tropistic behavior, and the inherit-

[1]Following Stone's practice, Munn speaks of mating, maternal behavior, etc., as examples of "congenital behavior" It is easy to sympathize with the desire to avoid the smoke of controversy over instincts which probably prompted Stone to adopt this expression, but since "congenital" has the well-established meaning "existing at or from birth," it does not seem to be the correct term to use here as an alternative for "instinctive"

ance of activity level, savageness, maze-learning ability, effects of training, etc The second chapter has two sections, the first dealing with general activity and its determinants, the second dealing with the relative strength of various motives as measured chiefly by obstruction-box and choice-box methods. Chapter III, on "Sensory Processes," opens with a brief résumé of what is known of the sense-organs of the rat, then describes and evaluates the various methods and apparatuses used to study sensory processes in the rat, and finally summarizes the experimental data available on visual, auditory, olfactory, gustatory, cutaneous, and proprioceptive functions. Chapter IV deals with the rather particular problem of "The Rôle of Sensory Processes in Maze Behavior" This chapter opens with a discussion of the logic of experiments on this topic, and then presents the really notable range of studies that have been conducted on this problem, including studies of direction orientation and of effects of maze rotation Chapter V, on "The Learning Process," is very naturally the long chapter[8] of the book It opens with a discussion of methods of measuring learning and of the reliability of these methods, then briefly considers methods of plotting learning curves, and finally gives a 70-page summary of experiments seeking to determine what factors govern learning and retention[9] Chapter VI, on "The Nature and Theoretical Basis of Learning," despite its primarily theoretical purpose, does include a very considerable amount of experimental material not covered elsewhere in the book Three general interpretations of learning are considered—exercise and effect theories, configurational theories, and conditioned-response theory The third of these is defended as the most satisfactory. Chapter VII, on "Symbolic Processes" (one wonders why this chapter did not precede, rather than follow, Chapter VI), deals mainly

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[8]Except for this chapter and the brief seventh chapter, all of the chapters are of about equal length.

[9]Despite the general thoroughness of this chapter, one topic which it seems that Munn has considered in too incidental a manner is the topic of what factors determine the relative difficulty of different mazes and of the different elements within any given maze Inasmuch as mazes are used so extensively, this topic is one of obviously great methodological significance Most of the material on this topic is recent (and some of the important parts of it still unpublished), but there is really some valuable material available on this matter If one combines the principles of (1) direction orientation, (2) goal gradient (as regards error elimination), (3) anticipatory errors—i e, particularly, the tendency to make, earlier in a run, the last turn required before food is reached (Hull, Spence—also forthcoming work by Buel), and (4) forward-going tendency (Dashiell and Bayroff) or centrifugal swing (Schneirla, Ballachey and Krechevsky—also forthcoming work by Ballachey), there is really the basis for a discussion of prime importance in this chapter However, Munn mentions the third and fourth of these only incidentally (p 337 and p 196), and the first two are discussed only as bearing primarily on other problems (see pp 193-197 and 335-339).

with delayed reaction, multiple choice, and double-alternation temporal-maze experiments The first and third of these three methods are interpreted by Munn as valid means of testing for symbolic processes

Two extensive bibliographies conclude the book The first, which gives about 300 references to studies of animals other than the rat, is classified roughly in accordance with the outline of the book, except for a first section on "general treatises" and a final section on "social behavior." The second bibliography contains 670 references on the behavior of the rat, including references up to August, 1932, and is arranged alphabetically by authors. The book is well indexed, both as regards authors and subjects.

In addition to the excellent features of the book already mentioned, one further one particularly deserves mention—namely, that the book is definitely critical and evaluative, rather than being a colorless summary of one study after another. Nevertheless, Munn preserves a very fair and impartial air in the discussion of the many controversial subjects, both large and small, with which the book deals Munn does indicate what his opinions are, and in some cases they are opinions that will not be shared by all workers in the field, but his whole discussion of such matters is judicial rather than partisan

On the matter of accuracy, also, the book is an excellent piece of work A very few scattered errors are to be found, but in a book covering such a vast range of experimental data it would be surprising if there were none

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The remainder of this review, despite the fact that the contents of the book have been indicated only rather grossly, and despite the fact that the good qualities of the book have been indicated only in very abstract terms, will be devoted to several adverse criticisms, particularly of Chapter VI. Inasmuch as this book almost certainly will receive extensive use because of its decided excellence as a text and reference work, it seems to the reviewer all the more important to criticize carefully the few scattered features in which the book is unsatisfactory.

The first adverse criticism is a minor one, and moreover is a criticism not so much of this book as of a general usage in psychological publications In the text proper, Munn has followed the practice of referring to sources by citing in each case the author's name and the bibliographical number This practice is of course superior to citing the bibliographical numbers alone, but it is annoyingly inferior to the practice of citing references by author and year of publication [as, "Small (1899)" or "Small ('99)"] Especially in a work like this, covering such a span of years, one is forced to turn to the bibliography time after time merely for the dates of publication of various studies, so that one can place them in their proper historical setting Where the other system of reference is used, this information is given directly in the text with no increase in space, greatly

to the aid of the reader, and greatly to the convenience of the author in the preparation of his manuscript. The continued use of the conventional system of reference by psychologists has no other justification than the very weighty reason, "it always has been done"

A second objectionable feature of the book is that the discussion *of many studies is closed with a comment to the effect that the differences found* were or were not "*statistically* significant," much as though the proper climax of an experiment was the determination of critical ratios Of course, the responsibility for this feature of the book does not rest primarily on Munn, for such a conclusion is the only goal that many animal studies have reached or even attempted to reach, and where the original author attempted further to calculate the *relative significance for behavior* of the factors studied, as Tryon has done, Munn has availed himself of this material It *would have been very desirable, however, for Munn, somewhere in the* book, to have indicated the naiveté of the view that the end and purpose of psychological experiments is to find reliable differences, and to have stressed that, instead, the reliabilities of averages, differences, etc., are merely tools Someone has remarked, perhaps somewhat unjustly, that continental psychologists, never having heard of statistics, throw up their hands and give thanks whenever they find a difference We in this country have become a bit more sophisticated and lift our arms in jubilation whenever we find reliable differences. *We need to advance at least one step further and regard a study as adequate only when it has succeeded in determining,* as one might say, the extra-statistical significance of its measures—i e, only when it has succeeded in discovering the relative importance of the various factors that determine behavior in the given type of situation

The most important criticisms demanded by the book, however, concern the interpretation of learning given in Chapter VI. This is the one portion of the book which falls appreciably and seriously below the general level of the book This chapter is *a curious mixture, however* On the one hand, *it summarizes in quite an able way a large range of experiments* showing that rats learn to select spatially and temporally shortest *routes, that rate* of error elimination is conditioned by the sort of motivation and the appropriateness of incentive used, that rats can make certain readjustments of response not derived as such from their past experience, that frequency does not necessarily produce learning, etc On the other hand, however, Munn waves aside all of this material as of relatively slight interest for the problem of the nature of learning, and builds up, instead, an interpreta*tion of rat learning in terms of conditioning,* even though he cites virtually *no directly relevant material supporting such an interpretation* It is this theoretical phase of Munn's discussion which *will be considered in the re*mainder of this interview.

Five main principles form the basis of Munn's interpretation of learning. They are

1. Most of the theories which attempt to explain learning in the rat are defective because, rather than being explanations, they are merely descriptions of the learning activity and of the conditions thereof, and because, rather than explain, they often serve merely to blur the experimental facts themselves (Thus, see pp. 302-303, 309-310, and 339.)

2 The only really satisfactory explanation of learning would be an explanation in terms of the neural processes involved. (See pp 302-303, 310)

3 ". . . learning may be described as the elimination of ill-adapted acts and the fixation of adaptive acts" (p. 352)—i e, learning is the building up of a mosaic from fragments or particles of behavior present in earlier trials

4 To account for this selection and elimination, "The conditioned response theory of learning, despite its apparent simplicity and its other defects, is probably the most adequate yet offered" (p 352, see also pp. 346-351)

5. Learning may be defined only in terms of the directly observable changes in performance—learning *is* the *immediately-visible changing* of the performance of the animal (This principle is expressed, not so much by explicit statement—though see pp 314-315, 331, and 352—as by the fact that, though Munn presents rather completely the experiments on latent learning and expresses no objections to the techniques employed therein, he does not utilize the interpretation of learning which these experiments suggest)

In order to evaluate these principles, it may prove helpful to examine first the question of what is meant by explanation, since a lot of Munn's statements seem to result from the fact that, apparently, he regards "description" and "explanation" as being two entirely different things. Description, he seems to believe, is achieved much more easily than explanation, but is not nearly as valuable; generality is desirable in explanations, but in description the use of generalized terms is a matter of using "blanket terms" that "hide the observable facts themselves"; etc

When we scrutinize this problem of the nature of explanation and of description, what we discover does not harmonize at all with Munn's discussion Description does not prove to be one thing and explanation something *else*, but *rather*, *as countless writers have pointed out*, *explanation* is description, and nothing but description Or, to express the matter more adequately, the explanation of a given phenomenon is merely the description, as exactly as possible, of the conditions essential for its occurrence In order for an explanation to be satisfactory, three things are necessary (1) the description should include all of the conditions which are essential for the occurrence of the phenomenon in question; (2) the description should not include any items which are merely incidental, and not in themselves

essential, and (3) the description should be given in as generalized a form as possible.

Now, since the conditions necessary for a given type of psychological activity may (some day) be specified fairly completely by descriptions of the immediate situation, past training, etc., required *to produce that activity, it is obvious that fairly adequate explanation* is possible in psychology without resort to neurological terms. As a matter of fact, if we knew, for example, what neural patterns were present in complex human thought, and were not able to describe the training conditions required to develop those habits of thought, we would then have but a *very poor explanation of thinking, for we would have but a very incomplete* description of the conditions essential for the production of the phenomenon in question. The same would hold true for the many problems in animal learning It may be seen, therefore, that the only difference that might exist between explanations involving neural terms and those not involving neural terms would be a *difference of degree, since if we had techniques for identifying the* nervous processes involved in different types of behavior we might be able to work out a more exact specification of the conditions necessary for the production of particular acts But there does not exist any more fundamental distinction between ordinary psychological explanation and explanation *in neural terms Accordingly, the first objection to* Munn's *discussion* of learning is that there is no reasonable basis for the exclusive "explanatory" significance which he attributes to our dreamed-of explanations of learning in terms of the neural processes involved

The second objection to Munn's interpretation of learning concerns the high value which he *assigns to the conditioned-response theory* The basis for this objection may best be indicated by reference to the above discussion of the nature of explanation It may readily be seen that, of the three requisites of satisfactory explanation, as outlined above, the least fundamental is the third, important and desirable though it is. For, obviously, if an *explanation does not indicate correctly* what conditions are essential *and* what *conditions are non-essential for the production of a certain result,* it does not matter what degree of generality the explanation has Nothing is to be gained, and everything may be lost, by seeking for generality at the cost of either the first or the second requirements of explanations.

Now, admittedly, explanations of learning in terms of conditioning[4] do

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[4]A distinction needs to be drawn between two rather divergent *tendencies* in the utilization of the concepts of conditioning. *According to one tendency,* the main interest centers in the question of whether *the relatively specific* concepts of conditioning (*as initial generalization, experimental* extinction, spontaneous recovery, disinhibition, etc) *hold true for a broader* field of *learning than we now have evidence is the case* The other tendency, of which Munn's *statements are representative,* places the emphasis merely upon the *broad primary principle* of conditioning—that when a novel stim-

have the characteristic of generality However, on other scores the explanation of learning by the conditioned-response theory is not very satisfactory. In general, these three objections hold, in the order of their increasing importance. (1) Both the development and the functioning of conditioned responses are, in part, a function of the motivation present in the animal at the time, but, although workers on conditioning employ this principle in their experimental work, they do not include it very explicitly in their formulations of the laws of conditioning, and do not make it the object of any particular research (2) As the work of Wever and of Warner shows, the conditioning formula predicts a result different from that which actually occurs in some learning situations—in some situations the joint presentation of a conditioning and of a novel stimulus finally causes the novel stimulus, when presented alone, to call out a response quite different in nature from the response which the conditioning stimulus itself had evoked. (3) The conditioned-response theory does not specify at all exactly the conditions which are necessary to produce learning For, though in one conditioning situation hundreds of repetitions are required to produce the conditioning, and in other conditioning situations merely one or two trials suffice, the conditioned-response concepts provide no specifications of the conditions under which the one or the other of these results may be anticipated. If the conditioned-response theory were an adequate interpretation of maze learning, discrimination learning, etc., it should provide the basis for predicting what mazes and what discrimination set-ups will be difficult and what ones easy for the rats to learn; etc Clearly, since the conditioned-response theory does not afford a basis for such predictions, it is a very incomplete description of the conditions necessary for learning, or else determinism is a myth

Before accepting the conditioned-response theory of learning, it seems desirable, therefore, to ask whether there are any other theories which provide a basis for more exact predictions To the reviewer it seems that there are several theories which individually have their advantages over the conditioned-response theory, and a combination of the advantages of all of these systems would seem to provide a considerably better interpretation. First of all, in comparison with conditioning, association has the advantage that it gives a somewhat more exact statement of the necessary conditions for learning in its principle (mentioned incidentally by Munn, quoting Hunter) that it is not merely the joint *presentation* of stimuli that causes association between them, but it is the joint *experiencing* of them

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ulus repeatedly is presented in close temporal juxtaposition with the biologically adequate stimulus for a response, the novel stimulus will eventually acquire the power to evoke the same response. What is being criticized here is the tendency to place a high value upon this broad primary principle of conditioning.

But association doctrine also is inadequate in that it provides only a very inadequate framework for discovering why certain things will be experienced together and other things not Gestalt psychology has improved on association theory by its development of a set of concepts that furnish the framework for further specifying the conditions which determine the ease with which learning occurs in different situations[5] Other improvements on association theory have been made by Tolman, both in his studies of how the factor of motivation enters into the learning process and in his more generalized concept of learning as a matter of the formation of "sign-gestalt-expectations" Or, somewhat similar to the latter is the redintegration notion of Hollingworth Either of these concepts has in it all of the positive material which the conditioned-response formula contains, and they are free also from the inaccuracies and some of the limitations of the latter Both concepts imply that the animal's response is not mediated by any rather mechanical bond between stimulus and response, but suggest rather that, after learning, the reaction that will be made will be a reaction to the redintegrated representation of the original situation *plus*, or *fused with*, the representation of the present situation Thus, they are not inconsistent with such findings as those of Wever and Warner

It is very odd that Munn should discard Tolman's concepts as "blanket terms" that "hide the describable facts themselves" (p 309), and accept an interpretation of learning as conditioning instead. As a matter of fact, Tolman's more elaborate specifications of the mechanisms and principles of learning furnish a far more specific description of the conditions of behavior than does Munn's description of learning as conditioning But, despite the fact that Tolman's terms are of more restricted generality, and even though, according to Munn's statement, they summarize actual experi-

[5]Munn dismisses the concepts of learning of Gestalt psychology merely with a discussion of insight in the sense of *sudden* seeing of relations, and with the following more general comment· "Some *Gestalt* psychologists have accused other psychologists of ignoring the fact that a theory of behavior must take cognizance of the integrative aspects of the animal's nervous system and of the environment That this accusation is not well-founded can be ascertained from the above discussion" (p 344). However, the "above discussions" do not seem to me to bear on the point. Apparently Munn is thinking of integrative effects in terms of correlations between stimuli and responses and in terms of associations established by training. However, the Gestalt psychologists have never objected to the view that, as a consequence of learning, the nervous system can function integratively even with utterly nonsense materials Rather, their point is that, as a result of the relation between environmental stimuli and the relatively spontaneous or inherent organizing tendencies of the nervous system, there are integrative effects (equivalence of stimuli, figure and ground organization, etc) which are relatively independent of learning, but which are basically *important* as determinants of transfer, of the ease or difficulty of the learning situation, etc

In conclusion, in view of the lengthy discussion which has been accorded the theoretical interpretations of this book, it is perhaps well to remind the reader that only a small part of the book has been concerned in this discussion. Munn's book is not primarily an attempt to develop theoretical implications, but is an attempt to give an accurate, meaty, and as-up-to-the-minute-as-possible account of a vast range of concrete experimental work. This task is one which Munn has accomplished in an exceedingly capable manner.

ROBERT LEEPER

University of Chicago
Chicago, Illinois

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SUBJECT INDEX